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21 Jun. 1866.

Orographic Geology;

OR,

THE ORIGIN AND STRUCTURE OF MOUNTAINS.

A REVIEW.

By GEORGE L. VOSE,
CIVIL ENGINEER.

"C'EST SUR-TOUT L'ÉTUDE DES MONTAGNES, QUI PEUT ACCÉLÉRER LES PROGRÈS DE LA THÉORIE DE CE GLOBE."
Saussure, Voyages dans les Alps: Disc. Prelim.

BOSTON:
PUBLISHED BY LEE AND SHEPARD,
149, WASHINGTON STREET.
1866.

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It is by no means intended to review in detail the works above mentioned, but simply to draw therefrom the latest ideas of leading geologists regarding the structure and the origin of mountains, as it is in these works that such ideas appear. On the other hand, we do not propose to confine ourselves to the above reports, but shall draw from the numerous publications of this and of other countries whatever seems needed to illustrate the subject.

It may appear to the general reader, that the pages are unnecessarily encumbered with notes and references; but the geological student will not so regard them. Indeed, to the latter the notes will most likely be of more value than the text; as they will save him the loss of time necessarily attendant upon the looking up of material widely scattered through numerous scientific journals,—an advantage apparent to any one who has done such work.

SALEM, Mass., March, 1866.

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OROGRAPHIC GEOLOGY.

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OROGRAPHIC GEOLOGY.

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OROGRAPHIC GEOLOGY.

The study of mountains has always been a favorite one with geologists, and very properly so; for in these vast masses of rock we are presented with an array of facts which we should in vain seek for elsewhere. It is by means of these upturned strata, ruptured and worn by rivers into deep ravines and valleys, that we are made acquainted with the underlying formations. As Saussure observed, it is not in deep mines that we are to study the foundations of the earth's crust, but on the tops of high mountains.

Many geological theories seem to have been formed, not so much for the purpose of explaining facts, as to avoid the trouble of investigating them. Sweeping generalizations have been made, to support which, all the favorable evidence has been adduced, while the refractory phenomena have been left alone; and what makes it worse, many of the boldest speculations have been put forth under names which carry so much weight that no one thinks of questioning the conclusions, or even of asking for the evidence. Thus, the ideas held by Humboldt, Von Buch, Carl Ritter, Beaumont, and Agassiz regarding the origin of mountains, it will appear in the sequel, are altogether without foundation: nevertheless, the facts furnished by these distinguished men are of immense value.

It is perhaps a necessary evil attendant upon the use of text-books, that in order to tell a plain story and to make their subject comprehended, they assume a completeness in the solution of problems which cannot possibly exist with our present acquirements. The result is, that readers of such get definite ideas, but incorrect ones; and afterwards, when they push their investigations beyond the region of text-books, they meet with new and troublesome facts, which make their ideas more correct, but less plainly defined. For example, nothing can be simpler than the standard explanation of mountain structure, which supposes a great wedge of granite to form the axis, upon the flanks of which the stratified rocks repose in an inclined position. This is quite definite, but, as will shortly appear, depends more upon the imagination of certain geologists than upon any thing in nature. Such explanation is a summary mode of disposing of certain supposed facts; but it ignores other facts, which, when closely examined, prove the whole to be no more than the "baseless fabric of a dream." Whoever goes for downright truth, will most often have to content himself with incomplete and unsatisfactory conclusions. But this should in no way discourage us; for correct theory and truth are one, and the truth can only be discovered by bringing our minds into accordance with it. The eminent Norwegian geologist, Keilhau, observes, "A complete and perfectly candid study of facts must often leave us with nothing but what is inexplicable;" but, as Professor Dana remarks, "the more reason for doubts we know, the better, if they actually exist."

There have been many geological theories put forth by persons possessing a strong fancy, but little knowledge of facts, to refer to which would be simply a waste of time. Omitting such, let us examine as briefly as possible those systems which, though unsatisfactory, deserve respect, both from the names under which they have appeared, and from the labor which has been expended in their illustration.

BEAUMONT'S HYPOTHESIS.

A distinguished French geologist, M. Elie Beaumont, proposed an explanation for orographic phenomena, which made its first appearance in 1829, and which has been presented to the public since that time under a variety of modifications, both by its author and by other geologists.[1] M. Beaumont supposes, that, in the history of the earth, there have been long periods of comparative repose, during which the deposition of sedimentary matter went on regularly and continuously; that between these periods there have been short ones of paroxysmal violence, during which a great number of mountain-chains have been suddenly thrown up, all of the ranges originating at one time being very nearly parallel, no matter how far distant from one another; that these paroxysms have occurred from the remotest geological epochs, and may even now astonish us by new catastrophes producing new ranges. The origin of these mountain eruptions is found in the secular cooling of the globe, the whole mass of which, with the exception of an envelope much thinner in proportion than the shell to an egg, is kept in a melted condition by heat, but is constantly cooling, and thus contracting. The external crust does not gradually collapse upon the shrinking nucleus, but becomes separated from the central portion during the long periods, and, when it gives way, falls in suddenly along determinate lines of fracture. At such times the rocks are subject to great lateral pressure, which crushes the rigid masses, and bends the pliant ones into elevations and depressions, thus producing the folds or waves called mountains. All these lines of fracture, and thus of elevation, made at one time, are supposed to be parallel, — by which is meant that they lie on different small circles parallel to the same great circle; and the various systems of parallel ranges are so grouped as to form a pentagonal network, upon which geometrical plan a vast amount of good mathematics has been wasted. The means by which the time of elevation of any given range is known, is the age of the sedimentary beds which are tilted up on the flanks of the central axis. Thus, if a mountain-range in any place has inclined beds of Silurian rocks upon its flanks, while the later Devonian rocks are undisturbed, then the elevation occurred between the Silurian and the Devonian ages; and, if a range in any other part of the world presents the same relations, it is inferred that the elevation took place at the same time.

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[1] Beaumont's views were communicated to the Academy of Sciences in Paris in June, 1829, and published in the Ann. des Sci. Nat., 1829; also in the Bull. Soc. Géol., for 1847, p. 864; still later in the twelfth volume of the Dictionnaire Universel d'Hist. Nat., 1852, — "Systèmes des Montagnes," — and the same published by itself in 3 vols. pp. 1543. It is examined by Mr. Hopkins, in his address to the Lond. Geol. Soc., and published in the Quart. Jour., vol. ix., 1853. It is given at length by Professor Nichol, in Johnston's Physical Atlas, pp. 5-8; and discussed in Lyell's Principles, chap. xi.

LYELL'S OBJECTIONS.

To the above, Mr. Lyell objects, that there is no scale by which to measure the time which elapsed between the deposition of any one formation and the succeeding one, and hence that there can be no such thing proved as contemporaneous elevation.[1] Again, M. Beaumont refers to the Longmynds, a small range in Shropshire, England, as having been elevated after the Cambrian and *before* the Silurian; and he refers the elevation of nine other ranges in France, Germany, and Sweden to the same time, because they are parallel to what he terms the typical Shropshire range: but both Mr. Murchison and the Government Geological Survey of Great Britain have plainly shown, that this small chain, the Longmynds, was not elevated until *after* the Silurian. Thus, if the other ranges *are* parallel to the typical one, their parallelism and contemporaneity have not the relation assumed by Beaumont. To avoid these difficulties, M. Beaumont has, since first putting forth his theory, so modified it as to destroy the only claim it ever had to attention, — its simplicity; inasmuch as he has multiplied the number of successive upheavals, and asserts that new lines of elevation sometimes take the direction of old ones, thus destroying entirely the use of parallelism as a time boundary. Again, Mr. Lyell justly observes, that the theory of a sudden elevation of mountains deprives us of that slow denudation which in all cases must have occurred to produce the present surface of the continents. In judging thus of M. Beaumont's plan, we must be careful not to do him the injustice of allowing him no part in the formation of a theory of mountain structure. He has furnished us an immense mass of facts regarding the disposition of mountain-ranges, and has thus made us acquainted with some of the conditions to be accounted for.[1]

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[1] Principles, chap. xi.

AGASSIZ'S IDEAS REGARDING MOUNTAINS.

Professor Agassiz's ideas regarding mountains may be gathered from papers recently published in the Atlantic Monthly.[2]

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[1] Mr. Hitchcock, in his Manual, p. 193 (1859), thinks that M. Beaumont may have pushed his principle too far, but that "there is too much evidence of uniformity and even identity in the manner in which the different chains have been elevated, too much proof of occasional paroxysmal and vertical movements and of intervening long periods of repose, not to admit that law has presided over the phenomena." We can hardly understand why law is any less important for results produced gradually, than when produced suddenly. Mr. Hitchcock farther says, p. 194, that Mr. Lyell has "shown only that it is difficult to fix upon that point in the interval between two consecutive rocks when the convulsion took place, and that the fundamental principle of Beaumont's theory remains unaffected;" and, "in respect to the exact geological time when the elevation occurred, it is, to say the least, very probable that it took place just at the termination of the period during which the elevated rock was in course of formation; for such a convulsion furnishes, in many cases at least, the only known reason why its deposition was brought to a close." We think Mr. Lyell has shown the impossibility of fixing, within immense limits, the point at which the change took place; and it seems to us, that the frequent and total changes in the nature of the Appalachian strata, which for a vast depth are conformable, show that convulsion is by no means necessary for producing a well-marked separation between beds. We think, also, that Mr. Lyell has entirely overthrown Beaumont's doctrine of the relation between the age and direction of mountain-chains. The doctrine of the sudden elevation will be seen in advance to be opposed by many important facts.

[2] Vols. xi. and xii., 1863.

He teaches that the earth is a mass of intensely heated matter, on which rests a thin crust, that would shrink as the globe cooled, thus becoming folded and wrinkled, and, when the tension became too great, would be ruptured: that all mountains have been upheaved by great convulsions, which rent asunder the crust, and destroyed the animals and plants living upon it at the time; which paroxysms were succeeded by long periods of repose, when new beds were formed, and new races of animals were introduced:[1] that the height of mountains is an index to their age, the newest being the highest: and that this relation arises from the fact, that, when the crust was very thin, the resistance to the outburst of fluid matter was small, and easily overcome; but, when the crust became thicker and more solid, the force was allowed to accumulate until it was sufficient to heave up the vast masses of the Alps and the Himalayas. If this is not the same thing as M. Beaumont's theory, it resembles it very much, and is violent enough to satisfy the most determined opponent of the doctrine of long-continued gentle action advocated by Mr. Lyell. Professor Agassiz observes, that the upward pressure of a melted mass, if not too violent, or if the strata are thick enough to resist it, would lift the beds into an arch or dome; that a vertical action produces a regular dome; but that the pressure has often been oblique, and has thus made the mountain steeper on one side than on the other, in some cases even overturning a mountain on to its side. The width of the arch, he says, depends, in a great degree, upon the thickness and flexibility of the beds of which it is composed. Thin and flexible strata are readily lifted into a sharp and abrupt arch, with a narrow base; while the thick and rigid beds have been folded into a wider arch, with a broader base. In what follows, it will be seen that such a relation between the thickness of the beds and the amplitude of the arch is the reverse of that which exists, and that there is a much more philosophical mode of accounting for curved strata than the above. Besides, as Mr. Rogers observes, an upward movement would *extend* or *stretch* the crust; while it is a generally admitted fact, that curved strata have been subjected to severe *compression*. Additional evidence will be seen hereafter, altogether opposed to such an origin of curved strata as assumed by Professor Agassiz.

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[1] It is satisfactory to have a statement plainly made; and Professor Agassiz leaves no room for doubt as to his meaning. "The crust of the earth," he says, "has had its storms, which have thrown up waves twenty thousand feet high, and fixed them as they stand." — "Mountains are but gigantic waves raised on the surface of the land by the geological tempests of past times." He supposes the Alps, Caucasians, and Himalayas to have been elevated at the same time by a gigantic rent across two continents. With this view we suppose Carl Ritter and Von Buch would concur, as the former (Geographical Studies, Gage's trans., 1863; pp. 323–4) holds that subterranean volcanic fires have not only heaved up islands, but continents also, and that the largest mountain-ranges are the result of a former wide-spread volcanic action; and the latter, that mountains are produced by the violent protrusion of molten rock, which broke forth through vast fissures in the sedimentary beds, which were thus tilted up, and subjected to great lateral pressure, in some cases sufficient to overturn the strata; and he supposes that this lateral action from the Alps was enough to produce secondary parallel fissures, through which came the Jura (Hoffman's Review of the Life and Works of Von Buch, Edin. Phil. Jour., vol. xxxi. p. 226). Humboldt also holds the sudden elevation of plutonic masses, and agrees with Beaumont, that there is no reason why, in future times, subterranean forces may not add new mountain systems to those we already have (Cosmos, vol. i. pp. 304–6. Bohn's ed., trans. by Otté).

CHANGE OF OPINION CONCERNING THE WHITE MOUNTAINS.

The doctrine of mountain structure, which, without regard to the cause of the phenomenon, holds that a fluid or a solid mass of granite was forced up so as to form the axis of a mountain-chain, upon the flanks of which the stratified beds rest in an inclined position, has been very generally taught, and is still held by many. Dr. Jackson, in his Geology of New Hampshire, considered the White Mountains to be a great axis of elevation, by which the sedimentary strata on both sides had been elevated and metamorphosed; and of Kearsarge, at North Conway, he says that it was protruded in a pasty condition.[1]

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[1] Geol. of N. H., p. 164, and sheet of sections; and Am. Jour. Sci., 1843, vol. xlv. p. 146.

Professor Hubbard called the top of Mount Washington a cap of mica slate carried up *en masse* by the granite nucleus.[1] Professor Silliman refers to the same group as a series of stupendous granite mountains elevated in ancient times.[2] They are now considered by leading geologists to be highly altered stratified rocks, probably of Devonian age.[3] Mount Katahdin, in Maine, which rises abruptly to upwards of five thousand feet above the sea, has also been described as a mass of erupted granite; but Mr. Charles Hitchcock reports it as consisting of a light-colored, bedded granite in the middle part of its height, but capped by a reddish granite, also bedded with a well-defined dip, and resting upon the lighter granite below, "very much as one sedimentary rock upon a conical peak caps another of different composition."[4] Dr. Edward Hitchcock, sen., in his publications generally, gives a sketch of the old granite axis: but, in his latest work,[5] he observes, that such axes do not appear in the White, Green, or Appalachian Mountains; that the stratified rocks do not generally slope away from the apices of granite hills, as if lifted up by the protrusion of the granite, but that in New England granite shows itself more often at the base than at the summit: and he concludes, that some more general cause than the presence of the massive rocks is needed to explain the displacement of the bedded ones; that the unstratified masses have partaken of the general movement, but have not caused or controlled it. Mr. Dana[6] remarks, that mountain-chains are not made of igneous ejections, except occasionally in some small portions. Mr. Jukes[7] says, that, in a personal examination of the large granitic tracts of the West of England and South-east of Ireland,

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[1] Am. Jour., vol. xxxiv. p. 121.
[2] Am. Jour., 1838, vol. xxxiv. p. 79.
[3] Dana, Lesley, Hunt, Rogers, Hall, and Logan.
[4] Report on the Geol. of Maine, 1861, p. 397.
[5] Geol. of Vermont, vol. ii. pp. 572-3.
[6] Manual, p. 722.
[7] Manual, pp. 310 and 349.

Central France, Newfoundland, the Cape of Good Hope, and Australia, he never found a granitic mass forming a true geological axis; and he says, that, if molten rock caused *any* motion in the rocks above it, it would be a movement of depression in the part from beneath which the molten rock came, and not of uptilting in the rocks above it, so that the rocks would dip *towards* and not *from* the molten mass.[1] Professor Rogers objects to the idea, that a granite wedge can force up the bedded rocks: for he observes, that molten matter cannot introduce itself in the form of a wedge until a fissure from below is opened; that some force must first crack the crust, and then molten matter may act as a narrow wedge or key, but that such plates must necessarily be narrow, and have the shape of walls with parallel faces rather than wedges broad at the base; also, that they will be found chiefly in the synclinal flexures of the strata, which are concave upwards, and that, where a rupture has taken place in an anticlinal wave, the injected mass will be larger at the top than at the bottom.[2] A true conception of the formation of a mineral vein or dyke, he says, will represent it as the consequence, and not the cause, of the rent it fills; the real process being, not a protrusion of fluid matter through the crust, breaking it in its passage as a solid wedge might, but a pumping of it into a rent already made.[3] Mr. Rogers also objects to a solid wedge of granite being thrust up through sedimentary beds, since it is not possible to imagine such a slipping of the assumed granite wedge past the edges of the strata confining it. and we have no evidence of that discontinuity between the wedge and the strata which such a movement should produce, but the contrary.[1] There is certainly no reason why solid granite should take the form of a wedge; and, if it happened to have such a form, it could move only by the application of some force from below, — an operation of which we have no evidence. We have perhaps said more than was necessary upon this matter, since Hitchcock, Dana, Rogers, Hall, and many other geologists, do not now believe in granite axes; but the ideas of Beaumont and Agassiz certainly require such, if indeed they be not the very foundation of their hypotheses.

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[1] Jamieson, in Proc. Brit. Assoc. for 1859, p. 114, says that the sedimentary rocks of Aberdeenshire, in Scotland, *do* generally dip towards, or underneath, the granite.

[2] We cannot understand how there can be any such thing as a vein or dyke of injection from *below* in an *anticlinal* flexure, though a rent in the curve convex upwards may be filled from *above* by aqueous deposition. We shall refer to this matter again.

[3] Prevost (Bull. Soc. Géol., 1839-40, t. xi., p. 186, says, "Que les matières ignées (granites, porphyres, etc.) loin d'avoir soulevé et rompu le sol pour s'échapper, ont seulement profité de solutions de continuité qui leur ont été offertes par le retrait et les ruptures, pour sortir et s'épancher au dehors."

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[1] Mr. Lyell, however (Manual, Am. ed., 1861, p. 583), speaks of breccias formed along the line of junction between granite and sedimentary beds, which are supposed to have been made by the protrusion of a solid mass of granite breaking through the shales, sandstones, and limestones.

Saussure asks (Voyages, § 1302) how the Alps could have been upheaved by central fires, and such heat have left no trace of its action upon the rocks, which, he says, he carefully sought for and could not find; and he remarks the well-known fact, that the central axes or highest ridges are not massive granite, but gneiss, — *granit veiné* (§ 1774).

Prevost, as before cited, says, "Que le relief de la surface du sol est le résultat de grands affaissements successifs, qui par contrecoup et d'une manière secondaire ont pu occasionner accidentellement des élévations absolues, des pressions latérales, des ploiements, des plissements, des ruptures, des tassements, des failles, etc., mais que rien n'autorise à croire que ces divers accidents ont été produits par une cause agissant sous le sol, c'est-à-dire par une force soulevante."

Mr. Dana remarks (Am. Jour. Sci., 1847, vol. iii. p. 180), that the expansive force pent up in the interior can only act against the incumbent pressure by expanding the fluid mass, and causing it to swell upwards: it can give no eruptive force to the igneous fluid.

Mr. Lyell says (Principles, chap. xi.) that it is not the magnitude of the effects, however gigantic, which can inform us, in the slightest degree, whether the operation that has produced the great physical features was sudden or gradual; that it must be shown, that slow forces could never, in any series of ages, give rise to the same result. Mr. Lyell's remarks upon "creeps" in coal mines (Manual, p. 52) are important, as showing that gradual operations produce results which seem to be due to sudden and violent actions, while those operations which have really been quick in their movement produce a uniform and non-paroxysmal appearance. We do not suppose, that infinitely small forces, however long continued, can produce the results of great and sudden ones; but we have in nature movements which appear gentle, because so slow in action, while in reality they are enormous. Such, for example, would be the gravity of a mass of rock fifty thousand feet deep.

THICKNESS OF THE EARTH'S CRUST, AND INTERIOR CONDITION OF THE GLOBE.

There has been expressed, in what precedes, the idea which is very generally held and taught, that the globe is a mass of molten matter, covered by a comparatively thin crust. By the effect which the earth has upon the other heavenly bodies, we know that its mean density is about double that of the most dense part of the crust, *i.e.* of solid rock: thus it would increase in density as we go below the surface. Again, we know that there is a source of heat other than that in the sun by which the interior of the globe is affected: the evidence of this is the ejection of heated gases, water, and melted rock from earthquakes, hot springs, and volcanoes; also the fact that the temperature increases in mines, as we descend, at the rate of from one to two degrees Fahr. per hundred feet. This increase would, it is estimated, at a depth of fifty miles, be sufficient to melt all of the known rocks. But there is another element to be regarded in this matter, — pressure. Water will boil on the top of Mount Washington at 200° Fahr., while at the sea-level it needs 212°; and we may infer, that, under a yet greater pressure, it would refuse to boil at a much higher temperature. It has been suggested, that what is thus true of a liquid passing into a vapor may also be true of a solid passing into a fluid. This may be or may not. If a body expands upon cooling, its melting-point is lowered by pressure; but, if a body contracts on cooling, then its melting-point is raised by pressure. Thus, the melting-point of ice is lowered by pressure, while the boiling-point of water is raised by pressure, as water expands both ways from about 40° Fahr. As far as we know about rock, it expands from heat and contracts on cooling; and thus its melting-point would be raised by pressure, and rocks would remain solid at great depths, notwithstanding a very high temperature. The most solid rocks contain water, not only chemically, but mechanically combined; and the effect of temperature and pressure on this water, we may suppose, would affect the condition of the rock. At present we lack the data which would enable us to get the joint effect of heat and pressure at any given depth; and thus the solutions which eminent philosophers have furnished regarding the general physics of the globe are of little value. Mr. William Hopkins[1] concluded, that the crust of the globe must be eight hundred or one thousand miles thick, to account for the known facts of precession and nutation. The attractive influence of the moon upon the earth's equatorial protuberance will, he says, be sensibly the same, whether the earth is assumed entirely solid, as in astronomical works it is, or whether it is supposed to have a fluid nucleus, and a solid crust eight hundred or one thousand miles thick; but, if the crust is much less than eight hundred miles in depth, the precession will be different from what observation shows. The calculation which leads to this result, however, supposes the shell to slip round on the nucleus, as it might do if the passage from solid to fluid was abrupt; but if, as we may suppose to be the case, the joint effect of pressure and temperature produces a gradual transition from the solid to the fluid part, thus a viscous mass, a thin shell, may so drag the fluid nucleus around with it as to cause the globe to behave astronomically as a solid body.[2] Professor William Thomson concludes, that, unless the average substance of the earth be as rigid as steel, it must yield, under the tide-generating influence of the moon, to an extent enough to sensibly diminish the actual phenomena of the tides; and, to enable the tides, precession, and nutation to be as they are, he thinks a crust of two thousand or two thousand five hundred miles in depth to be needed.[1] Mr. Houghton would have a crust less than seven hundred and sixty-eight miles, but does not consider the matter within the domain of positive science.[2] Mr. Hennessy makes it not less than eighteen nor over six hundred miles.[3] Archdeacon Pratt considers two thousand seven hundred miles necessary.[4] The geological manuals generally hold to a crust fifty or one hundred miles deep, and a fluid mass directly beneath, — as indeed they must to account for the outflows of molten rock which they assume to have occurred. Mr. Sterry Hunt supposes, that the solid crust rests upon deeply buried aqueous sediments, so permeated by water and heat as to be in a state of aqueo-igneous fusion; or a zone of plastic material between the shell and nucleus, which he thinks would offer an explanation of all volcanic and igneous phenomena; and that, at the same time, the crust would be affected through the medium of the plastic zone precisely as if the whole interior was in a liquid state.[5]

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[1] Phil. Trans., 1839, 1840, and 1842; and Proc. Brit. Assoc. for 1847.

[2] Jukes's Manual, p. 227.

The form of the earth is that which would result from the rotation of a fluid mass; and to us this is the only satisfactory mode of accounting for its shape. This fluidity must have

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[1] Trans. Roy. Soc. Edin., vol. xxiii., and Phil. Mag., vol. xxv.; also, Phil. Trans., 1864, vol. cliii. part ii. p. 573.

[2] Trans. Roy. Irish Acad., 1852.

[3] Phil. Trans., 1851.

[4] Phil. Mag., 1859, vols. xvii., xviii.

[5] Am. Jour. Sci., 1861, vol. xxxi. p. 412.

Poisson, in his *Théorie de la Chaleur*, gives conditions under which a fluid globe would commence to solidify at the centre. Mr. Hopkins says (Proc. Brit. Assoc., 1847), that, if the increase of temperature be great enough to overcome the tendency to solidify from increase of pressure, solidification will commence at the surface; but, if the effect of augmentation of pressure predominates over that of temperature, the centre will become solid first: and he supposes, that solidification might commence at the surface and work downwards, and at the centre and proceed upwards, until no portion was left fluid, except certain large cavities at the junction, which would become more and more isolated as been, says Mr. Hitchcock, an igneous one, since the solid matter of the globe is at present fifty thousand times heavier than the water.[1] The assumption that the crystalline rocks are all products of igneous fusion has also been supposed to be evidence to the same point. Such evidence may, however, be found defective; since many crystalline rocks, even granites, it is now well known, are not the result of any fusion whatever, and since the most compact rocks are known to contain water. The present state of physical and geological science does not allow us to see that clear connection between any deep-seated terrestrial operations which may exist and the superficial effects to which we have access, that is so desirable to be known. The more we examine any physical matter, the more do we become convinced that all of the various phenomena of nature are intimately connected. It is only by perfecting the parts, that we can hope to understand the whole; and we need not be discouraged, if, with our present knowledge, we are unable to establish the dependence of the phenomena of mountain structure upon any hypothetical interior condition of the globe. The facts regarding the subsidence of certain regions and the elevation of others, which are now going on, and the far more general fact, that the deposition of the sedimentary rocks must have been accompanied in many places by a vertical settling of the earth's crust

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consolidation went on, and which might serve as reservoirs for volcanoes, and which would act in the same way as Mr. Hunt's "zone of deeply buried aqueous sediment."

Professor William Thomson, in his paper on the secular cooling of the earth, above noticed, says that the thermo-dynamic law connecting temperature and pressure in a fluid mass not allowed to lose or gain heat shows that the temperature in the liquid will increase from the surface downwards, if, as is most probably the case, the liquid contracts in cooling; on the other hand, if, like water, the liquid near its freezing-point expands in cooling, the temperature, according to the laws of convection and thermo-dynamics, would actually be lower at great depths than near the surface, even though the liquid was cooling from the top.

[1] Manual, 1859, p. 314.

to an enormous extent (seven miles in some parts of the Appalachians), seems to leave no room for doubt of a flexibility in the earth's crust, which cannot be conceived to exist in a rigid globe, nor with a rigid crust of any very great thickness. Even Mr. Hopkins, who shows first that the crust must be at least eight hundred miles deep, when he attempts to account for the bending and folding of strata, starts with the "fundamental hypothesis," that portions of the solid crust, "in some cases of large horizontal dimensions," have at some time reposed on fluid matter capable of elevating and dislocating the overlying solid.[1] Such time, of course, would have been, geologically speaking, quite recent, as the highest mountains on the globe have been elevated during and since the carboniferous period.[2]

GEOLOGICAL FACTS ELUCIDATED BY THE BROTHERS ROGERS.

In 1842, the brothers Henry D. and William B. Rogers — the first of whom had been engaged upon the geology of Pennsylvania, and the latter upon that of Virginia — presented a paper to the Association of American Geologists and Naturalists, entitled "The Laws of Structure of the more Disturbed Zones of the Earth's Crust." The same was again published in 1857,[3] and finally in the last part of the Geology of Pennsylvania, at first above referred to. It is in this last work that the observations are recorded upon which the "Laws" are founded. It is impossible to do justice to Mr. Rogers and his associates in this work, for the immense labor involved in collecting and digesting the material for such a report, by simply referring to the conclusions which are drawn from it; but the results are briefly and clearly stated at the end of the volume, and a theory proposed to account for the phenomena described, which bears directly upon our subject.

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[1] Prot. Brit. Assoc., 1847, before cited.

[2] Whether we can go still back of the fluid state of the globe is doubtful. The nebular theory, in its general plan, certainly accords with known physical laws: gases *do* contract to liquids, and liquids to solids, and both by the loss of heat, or, rather, not by loss, but by changing the heat into some form of motion. Mr. Tyndall observes (Lectures on Heat as a Mode of Motion, Am. ed., p. 57), that the stoppage of the earth's orbital motion would develop an amount of heat equal to the combustion of fourteen globes of coal equal to the earth in magnitude; and that, if the earth should fall into the sun, the amount of heat developed would be equal to the combustion of five thousand six hundred worlds of solid carbon.

[3] Trans. Royal Society of Edinburgh, vol. xxi. pp. 431–471.

It is a general fact, says Mr. Rogers, that strata dip in curved, and not in straight planes. Wherever wide areas of the crust have been elevated or depressed, the strata will be found, except when disarranged by faults, to form regular waves, having an amplitude proportioned to their flatness. These waves are parallel to each other, to the general trend of the mountain system of which they form a part, and especially to its chief igneous axis when it possesses such. The remarkable parallelism of the adjacent flexures in an undulated region belongs, not only to the waves that are straight on their crests, but also to such as curve: nowhere, perhaps, is this constancy so well preserved as in the Appalachians. This zone, fifteen hundred miles long and one hundred and fifty wide, consists longitudinally of eleven different sections, six of which are straight, three convex to the north-west, and two convex to the south-east.[1] Notwithstanding the windings of this chain, each division is made up of groups of waves essentially parallel. In most parts of the Appalachians, there is one class of waves from fifty to one hundred and twenty miles long and several miles wide, a second class not more than ten miles long and a fourth of a mile wide, and a third class still smaller, which seem more like superficial corrugations of the crust than true undulations of the entire thickness of the formations.

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[1] The map accompanying Professor Guyot's paper in the Am. Jour. Sci., 1861, vol. xxxi., on the Appalachian Mountain system, will illustrate the above remarks to those to whom the Pennsylvania Report is inaccessible.

This structure is plainly shown in the Appalachians, and, it is believed, in all corrugated districts where the crust movements have been simple, and have acted in one direction only. Where the elevating forces have acted in different directions at different times, causing interference of the waves like a chopped sea, as in the Swiss Alps and the mountains of Wales and Cumberland, the undulations are disguised, and are with extreme difficulty made out. The individual flexures of a belt of undulated strata occur under three forms, which Mr. Rogers calls symmetrical, normal, and folded flexures. In the symmetrical flexure, the wave is broad and flat, the slope on each side of the axis being the same; in the normal flexure, the crest is more elevated, the amplitude is less, and the slope is steeper on one side of the axis than on the other; in the folded flexure, the crust disturbance is the most violent, the width still less, and the slope very much steeper on one side than on the other, being even vertical or inverted, *i.e.* so as to bring the lower formation uppermost. At the same time, the axis plane,[1] which in the symmetrical flexure is perpendicular, in the normal flexure is inclined towards the steepest side of the wave, and in the folded flexure still more so. Passing from the side of maximum disturbance, invariably the side of the greatest igneous action, shown either in plutonic eruptions, in crust dislocations, or in metamorphism of the sedimentary rocks, the flexures pass from the closely folded through the normal to the symmetrical form, at the same time decreasing in height and increasing in amplitude.[2] Regarded longitudinally, says Mr. Rogers, the waves flatten out towards the ends; and, as they do so, the form changes from the normal to the symmetrical type. Confirming this, Mr. Darwin[1] says of the Chilian Andes, that he was able to follow with his eye a ridge gradually becoming higher and higher as the stratification increased in inclination, from one end, where its height was trifling and the strata gently inclined, to the other end, where vertical strata formed snow-clad pinnacles.

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[1] The axis plane of the flexure is the plane bisecting the angle formed by the slopes on the opposite sides of the crest of the wave.

[2] These several forms may be seen in the ocean-waves as they roll up on to a beach. In deep water, one or two hundred feet from the shore, we see the gently sloping symmetrical wave, which, as it nears the land, becomes successively a normal and a folded wave, and finally is thrown over on to its side. In this case, the sharpening and throwing over of the flexures is due to the resistance offered to the motion of the water by the bottom: this increases in proportion to the mass of the wave as the water shallows; and the consequence is a holding back of the lower

Another marked feature in plicated regions is that rupturing of the strata called faults. These, Mr. Rogers says, almost never occur on the gentle waves, but in the most compressed flexures, which have snapped or given way at the summit, where the bend is sharpest, in the act of curving; the less inclined side being shoved up on the plane of the fault, which is generally parallel to, if it does not coincide with, the axis plane. The direction of these faults generally follows the run of the line of elevation of the mountains. The length and vertical displacement of the fault depends on the energy of the disturbing force. One of these rents in Pennsylvania is twenty miles long and twenty thousand feet deep; and there is one in Virginia eighty miles in length.

CONFIRMATION OF THESE FACTS BY DIFFERENT AUTHORITIES.

Professor Hitchcock recognizes the wave form and the parallel lines of elevation in his admirable section from Mount Washington to Lake Champlain;[2] and he considers the facts as confirming Mr. Rogers's idea of a force acting from the south-east, though the section of Mansfield Mountain presents the steeper dips on the eastern side, while Mr. Rogers claims a prevalent steeper dip on the western slope of the flexures as a principal feature in his system.[3]

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part of the wave, which amounts to the same thing as forcing forward the top.

[1] Geological Observations on South America, chap. vii.

[2] Geol. of Vermont, vol. ii. pl. xvii. fig. 4.

[3] Mr. Darwin (South America, chap. vii. p. 176) describes the Andes

In Europe, Mr. Rogers refers to sections from the south-east to the north-west, through the Taunus to Westphalia, or by the Rhine from Bingen to Remagen, or from Hunsdruck to the coal-fields of Liege, as showing an almost universal south-east dip, resulting from closely folded and compressed strata, which dilate in going towards the German Ocean. Passing, however, from Germany into Switzerland, he describes the ranges of the Jura as having a prevailing steeper dip on the side of the waves towards the Alps. The Alps he describes as a double system of plication. Deep in towards the higher central chains, the folding of the strata is excessive, the waves being as it were doubled up, and pushed entirely over outwards towards the north-west; whilst, as we proceed into the great plain of Switzerland, the waves assume the normal, and finally, at the outward base of the mountains, the symmetrical type. The same phenomena are found in going from the central axis of the chain into the plains of

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of Chili as consisting of several anticlinal and uniclinal lines, ranging nearly north and south. In the exterior, he says, the strata are seldom inclined at a high angle; but in the central lofty ridges they are almost always highly inclined, often vertical, and broken by a great many faults. In chap. viii. he describes seven or eight several "axes of elevation" between Copiapo and the western flank of the main cordillera. At p. 231, chap. viii., he gives a section north of Copiapo, in the valley of the Despabaldo, showing strata so closely folded as to be inverted; a section which resembles strikingly that across the Chamouni valley, as given by Studer and others (Geol. der Schwyz, vol. i. p. 175; and Edin. Phil. Jour., vol. xxxiii. p. 144). In vol. vii. part ii., Pacific Railroad Surveys, the geologist starts on Beaumont's theory, and the sections are drawn by a believer in the old granite wedges of elevation; but the proof is a little too much, since he gives in pl. iii. fig. 4 a succession of four several "volcanic axes," and the same in pl. iv. fig. 5. In pl. v. fig. 1, he gives a section from the Pacific to the Sierra Nevada, across the Sta. Lucia, San José, and Gavalan ranges, which shows six "axes of elevation." In the Proc. Am. Assoc., vol. x. p. 126, Mr. Blake says that the Sierra Nevada is not composed of one ridge, but of many nearly parallel ranges, enclosing elevated valleys; and he thinks he recognizes the same forces which are illustrated in the Appalachians, but on a larger scale.

Lombardy; the folds being, in the higher parts of the chain, pushed over to the south-east, and assuming a normal and symmetrical form in the lower lands of Northern Italy. From the phenomena of the Appalachians, Alps, and other disturbed regions, says Mr. Rogers, it is obviously a general law that the axis planes are not only all inclined in one prevailing direction, but that they dip invariably towards the quarter of maximum disturbance.

In a paper on the South-west of Ireland, Professors Harkness and Blyth state that the Devonian strata of Cork and Kerry are arranged in a series of hills, having long, regular ridges running nearly east and west, making a succession of "rolls," which appear to have been pushed over in a more or less northerly direction, thus inverting the position of the coal-measures and the carboniferous limestone.[1]

But the most satisfactory confirmation furnished to the arrangement of the wave form, as laid down by Mr. Rogers, is found in Murchison's Russia,[2] in which he describes the ridges of the Urals as gradually opening out with wider sweeps and broader valleys, as the central axis of the chain is left. Between Verch-Uralsk and Bieloretzk, the strata are thrown about in many rapid undulations, forming high mountains, steep slopes, and deep valleys; next we have the more expanded folds and wider troughs of Uziansk and Petrofsk; the undulations become wider, and the ridges decrease in altitude, as they recede from the axis, until the whole series ends in the west in the broad trough between the Akri-tau and Tcheke-tau; and Mr. Murchison says that one of the facts most worthy the attention of geologists in the Ural Mountains is the very frequently inverted dip of the strata where they approach the axis, the formations which we know to be of younger age overlying the older rocks. This phenomenon, he observes, far from being peculiar, is of frequent occurrence along the sides of other ridges; and, in an elaborate paper upon the Alps,[1] he describes the general form as consisting of long folds parallel to the main centre line of the mass. Referring to the section from Altdorf to the Rhigi, he observes that the flexures increase in rapidity and intensity as we approach the St. Gothard Alps, — in other words, the folds open out into broader and less complicated sweeps in going from the north slope of the Gothard as a centre to the flanks of the chain where they expand into the canton of Lucerne; and he concludes, that, so far as mere outline goes, the undulations conform to the wave-like progression laid down by Professor Rogers, — in other words, the steeper sides of the anticlinals are most remote from the axis, while the longer and less inclined slope of each wave faces the Alps. Mr. Murchison also confirms Mr. Rogers upon another point; as he says that a prevalent feature in the Eastern Alps is the faults and inversions by means of which, on lines parallel to each other, the oldest portions of each group have often been thrown up on the external side of the Alps, with its last-formed member let down, as it were, so as to be in contact with the oldest rock, with the appearance of passing under it. These fractures have occurred at the most bent part of the wave.[2]

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[1] Edin. Phil. Jour., 1855, vol. ii. pp. 245-62.

[2] The Geology of Russia and the Ural Mountains. London, 1845, vol. i. pp. 461-3 and 646.

Professor John Phillips, in an able paper on the Malvern and Abberly Hills,[3] which lie west of the Severn, in the southwest of England, gives four sections which confirm the statements of Mr. Rogers regarding the wave forms. The section just south of Ludlow shows a closely folded, inverted wave; the next section, through Ledbury and Woolhope, shows waves of the normal type; the section through the forest of Dean to the Usk shows an approach to the symmetrical form, which is more fully reached at Tortworth, near Bristol. This inversion of the sedimentary strata on the west flank of the Malvern Hills has been noticed by many geologists, and has received very extended examination by Mr. Phillips.[1]

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[1] Quart. Jour. Geol. Soc., 1849, vol. v. pp. 157-312.

[2] Murchison's Siluria contains many excellent examples of wave structure, among which there is none finer than that across the north end of the Thuringerwald, near Eisenach, where the Wartburg is seen upon the denuded summit of a vast anticlinal (1859, p. 334). The Quart. Jour. Geol. Soc., 1861, vol. xvii. p. 262, contains an extremely interesting section from Dunkeld to Blair Athol, showing a complete double system of inverted folds, which, if a correct representation, is well worth careful study, as the contortions are moderate, and the problem is presented in great simplicity.

[3] Mem. Geol. Sur. Gt. Britain, 1848, vol. ii. part i.

CONCLUSIONS REGARDING THE WAVE FORM.

We think that we are safe in concluding, notwithstanding the wide difference between the regularly undulated and restored sections presented by geologists, and the extremely irregular fragments which are found in the field, that the forces which have plicated the mountain regions have acted in such a manner as to produce a succession of waves, which are more and more closely folded, faulted, and disarranged, as we approach the centre of action; and that, as the undulations become more and more abrupt, they assume what Mr. Rogers calls the normal, and finally the folded form. We should bear in mind, however, when in the field, that long ages have been destroying the original shape of the flexures, and disguising the actions by which the strata were folded and broken, so that our first work is to reproduce the original waves.[2]

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[1] Horner, in Geol. Trans., 1811, vol. i. p. 281. The May-Hill anticlinal is an exception to the general rule, having its steeper slope on the east; and Murchison (Siluria, 1859, p. 105) reverses the slopes of the anticlinal between Ledbury and Eastnor, as given by Mr. Phillips, making the slope farthest from the axis the gentlest.

[2] Saussure remarks (Voyages, vol. i. § 332, p. 227), "Une foule de causes locales ont altéré la forme et la situation primitive des montagnes. Il s'agit de retrouver au milieu de ces ravages du temps, l'ordre et les lois qui présiderent à leur formation.

It has been doubted by good observers, whether the exceptions to the law of Mr. Rogers, regarding the direction of the steeper dips, were not of as frequent occurrence as the rule. Mr. J. P. Lesley, one of the best-

MR. ROGERS'S HYPOTHESIS, AND OBJECTIONS TO IT.

The hypothesis which Mr. Rogers offers for the explanation of the phenomena recorded is as follows: The wave structure of the Appalachians and other plicated zones is due to an actual undulation of the supposed flexible crust of the earth, exerted in parallel lines, and propagated in the manner of a horizontal pulsation from the liquid interior of the globe. The strata of a given region are supposed to have been subjected to excessive upward tension, arising from the expansion of molten matter and gaseous vapors; this tension was relieved by linear fissures, through which much of the vapor escaped; this sudden release of the pressure produced violent pulsations on the surface of the liquid below, which were communicated to the overlying crust, which was thus thrown into a series of flexures, that were keyed into the permanent forms they now have by the intrusion of molten matter. During these oscillations, the whole heaving tract is supposed to have been shoved or floated bodily forward in the direction of the advancing waves; and the union of this tangential action with the vertical wave-like movement is supposed to explain the

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informed geologists of Pennsylvania, says (Coal and its Topography, Philad. 1856, p. 180), "Carefully drawing the curves across the country to an equal horizontal and vertical scale, it will be found, that, in a large number of cases, whether a majority or a minority I am convinced that no one knows, there is no such thing as a normal curve at all, in either direction, east or west." Mr. Lesley describes the "Chestnut Ridge" anticlinal as very much steeper on the east than on the west; he also describes the "Broad-Top" region as containing five anticlinals, all having their steep dips on the east side. Mr. Rogers himself notices such cases, but says that they do not occur except where some smaller waves are riding on the back of a large one, or where the wave is connected with a fault or with some local interference; an explanation which Mr. Lesley thinks cannot apply to the Broad-Top case, and we should suppose not to Mansfield Mountain, in Vermont, as given by Mr. Hitchcock. Mr. Dana, in his paper on the geological results of the earth's contraction (Am. Jour., vol. iii. p. 183), seems to accept ***the fact*** of the steeper dips, as he offers a solution for the phenomenon.

peculiar steepening of the front side of each flexure, and a repetition of similar operations to occasion the folding under, or inversion, seen in the more compressed districts. Mr. Rogers thinks that no purely upward or vertical force, exerted either simultaneously or successively, along parallel lines, would produce a series of symmetrical flexures; and that a tangential pressure, unaccompanied by a vertical force, would result only in an imperceptible bulging of the whole region, or an irregular plication dependent on local irregularities in the amount of the resistance. It is hard, he says, to account for the phenomena by any hypothesis of a gradual pressure, exerted either vertically or horizontally; and a repetition of feeble tangential movements, he holds, could not successively accord, either in their direction or amount, and thus could not produce the grand flexures seen in nature.[1]

To us this hypothesis is altogether unsatisfactory and unphilosophical. In the first place, we are called upon to believe in the production of a gigantic force from the expansion of molten matter and certain gaseous vapors, which are assumed to exist; and, in the second place, that the rocky crust of the earth, which must have been rigid, or it would not have cracked and allowed the vapors to escape, and which no geologist supposes to be less than twenty-five miles thick, could be thrown into rapid pulsations; and, finally, that the intrusion of a melted key-stone can support an arch the height and breadth of which are measured by miles.

The condition most favorable for the generation of vapors beneath the crust would seem to be that in which a rigid shell reposes immediately upon an entirely fluid nucleus; but even then there can be no accumulation of gases between the crust and the nucleus. Whatever effect might be produced by heat coming from the centre to the circumference would be produced *in* the molten matter; and the only effect of such heat would be a general expansion against the overlying shell. But, at the same time, an amount of contraction must have taken place at the central parts, from the loss of that very heat which has expanded the parts next the crust; and, the total amount of heat being constant, the contraction at the centre would just balance the outer expansion, so that the result would be no pressure at all from below against the crust, and thus the globe would remain in a state of quiet as far as any internal erupting forces are concerned: a conclusion to which Mr. Lyell seems to have come, when he remarks, that "the ordinary repose of the surface implies an inertness in the internal mass which is truly wonderful,"[1] and also Sir John Herschel, who considers that every thing in the interior is motionless.[2] That the rocky crust, twenty-five or one hundred miles in thickness, perhaps much more, could be thrown into rapid undulations over a mile in height, and yet allow the several strata to remain well-defined and unshattered, is an assumption too monstrous to be entertained.

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[1] Under the hypothesis of a uniform lateral pressure, says La Bêche (Address in Quart. Jour., 1849, vol. v.), and a uniform thickness of crust, we do not see any thing to prevent the mass from being crumpled and folded, as in the Appalachians.

A uniform, gradual, horizontal pressure will throw a table-cloth into a succession of waves; and the more rigid the cloth, the more speedily do the more distant waves appear. If very thin and yielding, the second fold will not come until the first has been entirely compressed.

Mountains, says Professor Dana, have not been made by single heavings of the earth's crust, as waves may be thrown up on the ocean, but are the result of slow, long-continued, and often intermittent actions;[3] and he observes, that the injection of liquid lavas would be but a poor support for a chain of mountains; and injections of granite, he thinks, could not have acted so, as its crystalline nature shows that it cooled very slowly.[3] Mr. Lyell and Mr. Jukes both argue against the sudden production of faults; and many geologists agree with them, that, where the rocks on one side of a fault have been elevated above those on the opposite side, such elevation was an operation so gradual, that the upheaved side was worn away as fast as it came up; proof of which is, that, however great the vertical displacement, as shown by the chronological discordance on opposite sides of the fault, we never find one side higher than the other at the surface, except where some harder bedded rock has resisted the action of the denuding agent. Mr. Lesley says that he considers the Appalachian flexures, not as wave curves, but as curves of lateral pressure, but supposes the plication to have been assisted by such a vibration of the crust as would " shake the strata loose," and keep them in a plastic state, so that they could slip over each other;[1] an operation which, he says, is going on in the Andes at present. In such vibration we do not believe, nor in Mr. Lesley's remark, that " a single law of form, claimed by the Pennsylvania geologists, goes further to support Mr. Rogers's theory than all the objections do to overthrow it." Mr. Lesley says, that, if the normal curve could be established in connection with all of our anticlinals, the argument which it would afford for the fluid progression of the waves would be irresistible; that is, if the results produced are such as would follow from an assumed action, then such *was* the action, no matter how impossible it may be to account for it.

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[1] Principles, chap. xxxii.

[2] Quart. Jour. Geol. Soc., 1836, vol. ii. p. 548. Herschel, in his letter to Mr. Lyell, above quoted, asks, How came the gases to be condensed? why did they submit to be urged into liquefaction? If they were not originally elastic, but have become so by subterranean heat, whence came the heat, and why did it come?

[3] Am. Jour. Sci., 1863, vol. xxxvi. p. 231, and vol. iii. p. 180.

There is another fact which is entirely opposed to the production of mountains in the manner proposed by Mr. Rogers. We should suppose that such a force as he refers to, if it acted, would produce the effects described upon that portion of the overlying crust that was least able to resist the upheaving; but precisely the reverse is the fact. The whole depth of the Palæozoic formations in the Appalachians is from thirty thousand to fifty thousand feet, while the depth of the same formations in the great Mississippi basin is only from three thousand to five thousand feet; and yet the strata of the basin are hardly disturbed at all, while those in the Appalachians present the remarkable flexures before referred to: and this is a very general rule, that the higher the mountains are, the thicker are the formations, and the more extreme the foldings. This fact, which is also entirely opposed to the remarks of Professor Agassiz, will be more fully noticed hereafter.

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[1] Coal and its Topography, pp. 179, 180.

We do not consider the dynamics correct which teach that the whole depth of the crust has been bent or folded into waves, the top and bottom surfaces of which are parallel, and that those waves are held in place by such injections of fluid matter from below as would enter the comparatively small rents made by such bending. If we take a small green willow wand and bend it, we shall see the bark upon the concave side puckered and compressed, while, on the convex side, it will be stretched and pulled apart. Precisely the same thing tends to take place in any tabular mass, whatever the material and the size, when it is bent; and, in a great flexure of rock, the upper side of the crest and the lower side of the trough of the wave will suffer extension, while the under side of the crest and the upper side of the trough will be compressed; and, at some point between the compressed and extended sides, there will be neither extension nor compression, but a tendency to a separation between the upper and lower parts, by which a subterranean cavity will be produced: and, if one half of the corrugation above such cavity should sink into it, we might have a fault; but such fault would be produced gradually during the general depression of the whole mass, and thus also during the elevation of the secondary flexure producing the cavity. This action may be illustrated by taking a pile of sheets of letter-paper, and, not allowing the ends to slide over each other, depressing the middle; we shall see the upper sheets compressed and thrown into folds, which separate from the central ones, while the bottom ones are tightly stretched. The same operations may occur, but on a smaller scale, at the same time, in the secondary flexures where compression is going on. Under such conditions, the existence of any rents or fissures, extending entirely through the crust, would be extremely improbable; for where the convex side of the flexure would be split open, the concave side would be made more dense by compression. We think that, if any person will draw to an equal horizontal and vertical scale the Appalachian flexures given by Mr. Rogers, or the waved flexures given by any geologist, he will see at once the impossibility of believing such waves to be undulations affecting the whole thickness of the crust, even supposing it to be no more than a dozen miles deep, and the thicker the more improbable; and thus that the wave-form of the strata cannot be due to any pulsation of the crust, as Mr. Rogers assumes.[1]

MURCHISON'S HYPOTHESIS, AND OBJECTIONS TO IT.

Mr. Murchison[2] supposes that the emission of vast quantities of molten matter has furnished the material for the central ridges of mountain-chains, and that such emission left vast cavities, into which the ends of the fractured strata may have fallen, thus producing their inverted position by a simple movement of lateral depression towards the cavity. He also supposes that the molten matter may be likened to a series of subterranean waves moving from the flanks towards the centre of the range, in order to fill up the vacuity left by the central eruption. This conception, by no means remarkable for definiteness, is still further obscured subsequently, where he says, "We think the highly contorted and basin-shaped strata, which are so common on the western slopes of the Urals, are best explained by the lateral pressure of the sea, and sedimentary matter in the low country on the west, and the great uprise of the central ridge on the east; by which operation the masses were squeezed up between two resisting bands, and were thrown back, and partly inverted." We think Mr. Murchison's hypothesis appears not only utterly incomprehensible in detail, but also, in the general form, open to all the objections which apply to that of Mr. Rogers. The waves of Mr. Rogers, if we understand him, proceed from the centre of action towards the outer boundary of the mountain region, making the flexures less and less as the wave fades away. Mr. Murchison's waves proceed from the flanks towards the centre, and, we suppose, must, from some unknown reason, increase in violence as they proceed, inasmuch as the disturbance of the strata becomes greater and greater as we approach the axis.

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[1] Mr. Babbage presents an explanation for the normal form of flexed strata (Quart. Jour. Geol. Soc., vol. xii. pp. 366-8), which we do not think at all worthy of that distinguished man; and the same remark applies to Sir John Herschel's explanation of the origin of corrugations and cleavage, in the Phil. Mag., 1856, vol. xii. p. 197.

It is much to be desired, that illustrations should be presented in correct proportion. The most plausible theories often vanish before an accurate drawing: ideal sketches generally mislead, because they are drawn to enforce a particular point, which is developed at the expense of the rest. Thus Herschel, in the paper just above referred to, in order to show that sedimentary deposits would slide towards the central part of an ocean, draws a section representing an ocean a fourth part as deep as it is wide; but, if we draw an ocean one or two thousand miles wide and ten miles deep, it will be at once evident that no such action could possibly take place. So, too, Mr. Hitchcock, in his Manual, 1859, p. 318, represents the plication of the surface which would take place by a given contraction of the sphere: but any one who will take the trouble to calculate the difference in length between the outer and inner circles of his figure will find that the contraction which he represents would produce only one very small elevation, even supposing it all to occur at one point, and the material to be absolutely incompressible; and, if the plication was equally distributed around the circle, it would not be seen at all.

[2] Russia and the Urals, vol. i. p. 463; and App., p. 646.

LA BÊCHE'S REMARKS ON MR. ROGERS'S PLAN AND ON ALPINE STRUCTURE.

Sir Henry La Bêche, in his Annual Address to the London Geological Society in 1848,[1] commenting on Mr. Rogers's hypothesis, says, "If we confess, which we should have great difficulty in doing, that the external crust would conform in the manner supposed to the undulation of the waves produced in the fluid mass, we must, to produce each set of pulsations, have the accumulations of the vapors, the formation of the rent, and the closing up of the fissure so as again to permit another tension. It is supposed, that, at every pulsation, the beds were crumpled up and driven aside from the original rent, or others parallel to it; so that, unless the superficial part of the earth were diminishing, each impulse would drive the once-continuous beds further from each other, the intervening space requiring to be filled up by the liquid matter from beneath."[1] Mr. La Bêche concludes, from the facts of Mr. Rogers and of others, that lateral pressure of masses of the earth's crust against other masses has been the cause of the flexures, — with which we shall agree in the sequel; but he also holds that the whole thickness of the crust is represented by these flexures, — from which idea we have already recorded our dissent. Regarding the Alps, La Bêche remarks, that the strata present a crushed appearance, such as we might expect from the heavy pressure of the masses composing them against each other on a line corresponding with that of the main range; that, if the various dislocated parts were reunited, the folds flattened out, and the beds restored to the condition in which they were formed, the area now occupied by the Alps would have to be expanded to various distances parallel to the centre line;[2] and he concludes as follows:[3] "In looking at the flexures and dislocations of the Alps, we have to regard the mass of them; and, in so doing, we seem scarcely to arrive at the conclusion that the flanks have been driven outwards by impulses acting upon a fluid mass beneath, in consequence of tension along the central axis, but rather that the component beds were squeezed up from both sides against a main central line, so that the effects produced upon the partly flexible and partly rigid rocks would throw them into flexures, and break them in directions as if from a force acting from the centre outwards. The contorted, broken, and jammed masses, to avoid being squeezed up, would act with all their gravity in bending and breaking the rocks, so that towards the central axis the anticlinals would be steeper and more folded, with inversions, and become less so towards the extreme flanks." The importance of these ideas of La Bêche will be seen when we have considered Mr. Hall's hypothesis.

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[1] Quart. Jour., 1849, vol. v.

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[1] When we examine the plan of Mr. Hall, we shall see how the space left by the folding might be easily filled up without any fluid injections, and, as it seems to us, by the only conceivable mode.

[2] This shows plainly that elevation by any force from below had no part in the production of these mountains, since elevation can in no way compress, but would stretch the district raised.

[3] Address, before cited.

MR. DANA'S DOCTRINES.

Prominent among those geologists who have referred the elevation of mountains and the folding and fracturing of strata to the secular cooling of the globe, is Professor James D. Dana. He has applied this principle more in detail, and with far more skill, to the solution of the difficult geological problems, than has any one else. His views we will state as briefly as consistent with their importance.[1]

A melted globe, when cooling unequally, becomes depressed by the contraction on the side which cools last, and is therefore the thinnest. Nearly one-third of the face of the moon towards the earth is mostly free from volcanoes, while, on the other parts, the craters are closely crowded together: we may thus conclude that over this naked portion the surface first became solid, and has therefore cooled longest, and to the greatest depth. Thus the contraction from cooling would take place most rapidly over the thinner and more yielding volcanic portion; and, unless the ejections made up the difference, this part would become somewhat depressed. Upon our globe, the continents have, to a great extent, been long free from volcanoes, and are the thickest part; over the bed of the ocean, however, nearly all of the islands are volcanic: whence we conclude that the continental areas were first free from eruptions; they cooled first, and were and are the thickest, and the contraction affected most the oceanic areas. The great depressions thus began which are now the oceans, and which, for a long time, continued deepening (and may be now). Before these depressions were very deep, they would be too shallow to contain the seas; and thus the whole land would be under water, — and we know, that, in Silurian times, nearly every part of the globe *was* beneath the sea. As the sea-basins deepened, the water was lowered, and thus produced the apparent elevation of continents. It has been customary to suppose the sea-level to be fixed, while the land rose or fell. Elevations and depressions of the dry land have doubtless occurred; but why should we assume the land which lies beneath the ocean, and which forms two-thirds of the surface of the globe, to be rigid? In fact, the coral islands are positive evidence of the subsidence, on a large scale, of the bed of the ocean: fifty thousand square miles at least, and probably much more, have certainly subsided several thousand feet in the Pacific Ocean, and that, too, since the tertiary epoch.[1] The more prominent effects of contraction, then, will be depressions provided the contraction is unequal, apparent elevations as a consequence of the depressions, fissures and ejections of igneous matter therethrough, upheaval along the lines of fissure, and upliftings and foldings from lateral pressure. The result of the contracting of the ocean-bed would be seen at the junction of the contracting and non-contracting areas, *i.e*, at the borders of continents; and, in fact, mountain-ranges do generally follow coast-lines, — an arrangement particularly well seen in the Andes, Appalachians, and mountains of the Western United States. The contraction would go on most rapidly in the oceanic depressions, and the crust would be drawn down into the fluid mass. As the ocean-bed became thicker, the contraction would produce a lateral tension[1] against the non-contracting areas (the continents), which would occasion pressure, upheaval, &c. The supposition that the subsidence of the oceanic basins has caused the rise of the continental border ranges, Mr. Dana thinks, is confirmed by the proportion between the height of the range and the size of the ocean adjacent: and he says, that, all over the world, the highest mountains front the largest and deepest oceans; that the coast and mountain lines are, in general, turned to face the widest range of ocean: and he concludes that the extent and position of the oceans have determined the features of the land; that they have both been in progress, with mutual re-action, the oceanic depressions tending downwards and the continents upwards, from the beginning of the cooling of the globe; that, in a shrinking sphere, the large rind is more than enough for the contracted nucleus, and the drawing downwards of some parts must cause the bulging of others; that, if any large areas of the crust were sinking more than the rest, this very subsidence would necessarily push up the borders of the areas into folds, and the larger these areas (the oceans) the higher the border elevations. The mode in which Mr. Dana accounts for the steeper north-western dips in the Appalachians does not seem, however, very correct in its mechanics. He supposes that the crust of the ocean-bed exerts a thrust against the continental border which pushes it into folds[2] (perhaps as Mr. Murchison suggests in the case of the Urals); but it strikes us, that, if such a force *did* act, the re-action of the more stable continent would be the cause of folding the thinner and less resisting bed of the ocean.[1] To account for the steeper north-western dips, Mr. Dana supposes the compressing force to act upon a mass of sand and clay "very little or not at all indurated;" but, at the same time, he supposes that such a mass would act as a rigid system of levers, to produce, not only flexures, but flexures steeper on one side than on the other. His reasoning cannot well be made plain without the illustration he uses; but to us it is not at all satisfactory, nor do the facts bear out his hypothesis: for, in the case of the Appalachians, his force from the sea would produce waves with their steeper sides *away from* the ocean, while, in the Urals, it is the side *towards* the sea that would be the steepest; and, in the double system of the Alps, his ocean would have to act from the crest of the mountains outward in both directions to account for the steeper dips and inversions. Besides, Mr. Dana supposes the whole depth of the crust to be plicated, which we have already objected to, as it seems to us, on sufficient grounds. A general objection, also, to the plication of the earth's crust by the contraction dependent on secular refrigeration, may be stated here. The shortening of a given line on the earth's surface from a general contraction is quite insignificant as compared with the actual compression which has taken place in folded regions. If a quarter of the globe contracted laterally until the surface sunk eight miles, it would cause a lateral compression of only twelve miles and a half in the quadrant of six thousand miles, or one mile in five hundred, which, if the material was absolutely incompressible (which, of course, cannot be admitted), would produce only a single wave, a mile high and two miles

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[1] See Am. Jour. Sci., vol. ii. new ser. pp. 352–5, conclusion to "Lunar Volcanoes," "Origin of Continents:" also vol. iii. p. 381, "Origin of the Outline Features of the Earth;" same vol. pp. 94–100, "Origin of Continents;" and pp. 176–188, "Geological Results of the Earth's Contraction:" vol. xxii. p. 305, "American Geological History;" and p. 335, "Development of American Geology:" vol. xxxvi. p. 227, "On the Appalachians and Rocky Mountains as Time Boundaries in Geological History." Descartes, as early as 1644, referred the terrestrial dislocations to the cooling and contracting of the globe.

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[1] Dana, in Am. Jour., 1853, vol. xv. p. 157.

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[1] This is the word Mr. Dana uses: we think it should be *compression.*

[2] Am. Jour., vol. iii. p. 181.

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[1] Mr. Thomas MacFarlane, in the Canadian Naturalist, 1863, vol. viii. p. 474, observes, that the portion of the earth's crust most liable to be affected by subsidences, consequent on contraction, would be the thinnest. So far, however, as we know the facts, the thin parts are just those that have not been folded, while the thick ones always are plicated. We might thus infer that contraction was not the cause.

wide, in a tract five hundred miles in breadth. Again, contraction from cooling would have been most active in the most ancient times; while the mountains, so far as we know, have all been made since the deposition of the fossil-bearing rocks, — many of them, indeed, later than the carboniferous age; and, as the highest mountains are the most recent, this is as much as saying, that, as the force decreased, the result of such force increased. This objection Mr. Dana explains by supposing, that the contraction was, in the oldest times, relieved by folds and ruptures in the oceanic basins, but that later, when the oceanic portions had cooled and become more rigid, they refused to yield to such contraction, which was therefore obliged to exert itself upon the continental areas; but such a supposition does not explain the non-plication of the wide-extended but thin Silurian strata in the Mississippi basin, which, from a uniform contraction, we should suppose would be affected sooner than the vastly deeper beds of the Appalachian region. Besides, Mr. Dana himself tells us that the continents cooled first, and were and are the thickest portion of the earth's crust, and therefore must still be better able to resist pressure from contraction. Briefly to review the objections to the contraction theory, — the contraction has not been enough to account for the plications; the flexures occur in the part of the crust best able to resist the force, and not in those which are the thinnest and least resisting; and the latest mountains produced, when the contracting force was the least, are those which would have required the greater power to upheave them.

GENERAL REMARKS UPON GEOLOGICAL OPERATIONS.

Before presenting the hypothesis of Mr. Hall, it will be necessary to refer to certain general principles, — to certain facts of which there is no doubt, and which have an important bearing on the matter under examination. It is well known in geology, that there are vast tracts of the earth's surface consisting of bedded or stratified rocks, — rocks which have been deposited as chemical precipitates and mechanical sediments from the waters which held them in solution or suspension, as sand-bars and mud-banks are formed at the present day. Such deposits contain the remains of the animals which frequented the oceans at the time the beds were being made; and, by a study of these remains, geologists have been able to make the development of life a means of determining the period at which any given series of beds were laid down. The mineral nature of the rock is no proof of the time of its origin, inasmuch as coarse grits, conglomerates, and sandstones alternate with fine-grained shales, slates, and limestones in every conceivable manner. It is a well-known fact, that animal life does not, and, it is thence inferred, never did, exist at any great depths, — say over five hundred feet: it is shore-life, and not deep-sea life; moreover, the early life of the globe was marine and in no case land life. Swift-running or shallow and disturbed waters deposit the coarse-grained sediments; while mud, clay, and lime are the products of deep and tranquil seas. All of the sediments, except certain animal, vegetable, and chemical substances (which form an inconsiderable part of the sedimentary rocks), are the fragments of pre-existing rocks, worn into sand or gravel by the action of water. Therefore all the beds of sedimentary rocks have been made from some previous rocks, worked up by the waves of the ocean, carried to greater or less distances, and finally laid down in strata of greater or less regularity and thickness, and consolidated by heat, pressure, chemical action, or all combined, into solid rocks, which, at different times, have been folded, broken, faulted, and metamorphosed. The beds vary in thickness, in arrangement, and in mineral nature in different places. In one location, we may, in going downwards, pass successively through beds of limestone, sandstone, slates, or shales; while, at no great distance, any one of these beds may grow thinner, and finally vanish as we follow it, leaving the series without such formation. In other places, an additional bed will make its appearance, increase in depth, persist for a while, and afterwards thin out and disappear. Moreover, the same bed will often, as we follow it horizontally, change its mineral nature so as to become, in a greater or less distance, an entirely different rock. All such variations are evidences of change in the origin of the material, in the direction and force of the depositing current, and in the depth of the water. If the strata which have been deposited at different times in the Appalachian region were unfolded and simply piled in horizontal layers, the whole depth laid down since the lowest Silurian would be no less than forty thousand feet; while, in the great Mississippi basin, the same time, judging by the included fossils, produced not over four thousand feet. It is upon actions in progress during the deposition of these sedimentary strata that Mr. Hall's hypothesis depends. He commences by describing the material, thickness, and arrangement of the strata in New York, Pennsylvania, Ohio, Illinois, Iowa, and Wisconsin; and he next develops a connection between the thickness or depth of the deposits and the existence of folding and faulting in the same.

MR. HALL'S HYPOTHESIS.

We are accustomed, says Mr. Hall, to believe that mountains are produced by upheaval, folding, and plication of the strata; and that, from some cause, these lines of elevation extend along certain directions, gradually dying out on either side, and subsiding at one or each extremity. He then shows that the line of maximum accumulation of sediments has been along the direction of the Appalachian chain, and that the line of this mountain-range and of the ancient oceanic current which deposited the sediments is coincident: and he concludes that the line of the great Appalachian barrier is due to the original deposition of materials, and not to any subsequent operation dislocating the strata of which it is composed; that the gradual declination of the elevation of the country westward is due primarily to the thinning out of the formations, which have been deposited in such great force in the Appalachians; for no beds of later date than the upper part of the Mississippi series are piled on top of the Appalachians. To explain the folding and disturbance of the strata in the Appalachians, which, in the western basin, are horizontal; to show why the beds, which, in the east, are of immense thickness, should be closely plicated, while in the west where the depth is comparatively little, they remain undisturbed,—Mr. Hall offers the following. When large masses of sediment are spread along a sea-bottom, as originally along the line of the Appalachians, the first effect will be a yielding of the earth's crust beneath, and a gradual subsidence. We have evidence of this, first, in the great amount of material accumulated; for we cannot suppose that the sea was originally as deep as the thickness of these beds: indeed, the ripple-marks, the marine plants, &c., prove that the sea in which these deposits were a successively made was always shallow. The accumulation could thus only have been made by a gradual subsidence of the ocean-bed.[1] The greatest depression would be along the line of the greatest accumulation; and, in the direction of the thinning margin, the settling would be less. By this process, as the lower side became gradually curved, rents and fractures upon that side would occur, while the compressed upper surface would be wrinkled and folded. The sinking down of the mass produces a great synclinal axis; and within this, whether on a large or a small scale, will be numerous smaller synclinal and anticlinal axes. And the same is true of every synclinal axis: it will contain still smaller synclinals, or, if the folding is not sharp enough to actually make the smaller folds, the tendency will still be to produce the various phenomena of compression (cleavage, &c.). That the subsidence was periodical, we have the best evidence in the unconformability of certain beds; showing that the lower one was bent and disturbed before the deposition of the upper one. This hypothesis explains why the mountain elevations in disturbed regions bear in their altitude a much less proportion to the thickness of the formations, than the hills in undisturbed regions do to the thickness of their beds. The great weight of the thicker beds has caused them to subside so much, that a larger part of their depth is now below the level of the sea. This mode of depression, Mr. Hall thinks, offers a satisfactory explanation of the steeper dip on the north-western side of the Appalachian waves. The depression, he says, arising from the accumulation will be the greatest in the centre of the mass: the longest (most gentle) slopes would be towards the centre of the great synclinal; while the outer slopes would be shorter and steeper from the general movement towards the centre, such movement being equivalent to a pressure from the north-west towards the centre line. However much of indefiniteness there may be in this explanation, it certainly accounts for the facts, not only in the Appalachians, but also in the Urals, and in the double system of the Alps, which is more than can be done by any of the other explanations proposed: it agrees, also, completely with the movements suggested by La Bêche, and accounts for the various phenomena of compression so universal in mountain regions.[1]

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[1] We meet with a difficulty at the commencement of this explanation. If a large mass of sediment was put upon a crust capable of yielding, a subsidence would result: but, by the conditions assumed, we are not allowed to suppose that a large mass *could* be placed upon the crust; for the accumulation is said to occur always in shallow water. A deep and heavy mass cannot be accumulated in shallow water; and a thin bed of sediment would not produce a change of form in the crust unless the latter was exceedingly flexible.

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[1] Mr. Hall uses the illustration, already referred to, of depressing a bundle of sheets of paper, to show that the upper strata would be compressed, and the lower ones extended; and he also suggests, that the subjacent fluid matter might fill the fissures beneath. Mr. Phillips, in his excellent paper on the Malvern Hills, already referred to, confirms the ideas of Mr. Hall on some points. He observes, that the sinking of the Paleozoic strata during the accumulation of their upper members is the first step, and the subsequent elevation a second: he supposes a series of symmetrical flexures to be formed, and one end of the series afterwards to be elevated, the effect of which is to make all of the waves steeper on the side away from the elevated end of the system, or the point of maximum disturbance; and he also gives a sketch of the man-

It nowhere appears, Mr. Hall continues, that this folding and plication has contributed to the altitude of the mountains: on the other hand, the more extreme the plication, the more it will conduce to the general degradation of the mass whenever subjected to denuding agencies. The number and abruptness of the foldings will depend on the width of the zone which is depressed, and the depth of the depression, which last depends on the amount of accumulation. The most abrupt waves are split and weakened at the arching, and thus made liable to wearing away. So much is this the case, that the anticlinals are often worn down so as to form low lands or deep valleys; while the synclinal, protected by the downward curving of the bed, remains to form the mountain summit. Snowdon, in Wales, is 3,571 feet high, and, though at present above the surrounding land, is a synclinal mountain, and was once the trough of a wave, of which the adjacent crest has been worn away.[1] Mount Washington, in New Hampshire, is another example of a synclinal mountain; and Greylock, at North Adams, Mass., has the same structure.[2]

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ner in which a set of smaller undulations may occur within a large synclinal. The folding and faulting of the Malvern district, Mr. Phillips observes, from the earliest Silurian to the latest carboniferous, has depended upon three several actions: first, extensive and long-continued depressions of the ancient sea-bed; second, a great local elevation, affecting the same or parts of the same sea-bed, with a series of waves; and third, fractures through these undulations. Mr. Lyell, in his Travels in North America (vol. ii. p. 11), says that it is probable that the Silurian, Devonian, and the carboniferous strata, forming the Appalachians, underwent the principal movement of upheaval and subsidence to which their prevailing structure is due, at a time when they were still submerged beneath that ocean in which they were originally formed; and he also remarks, that the last series of movements which upraised the continents was quite distinct from those prior movements which threw the ancient strata into their inclined and curved position.

[1] See Ramsay's section at the bottom of Murchison's map to "Siluria;" also the section on page 92, Siluria, ed. of 1859.

[2] Hitchcock's sheet of sections, in Report on Geology of Vermont.

Mr. Lesley (Coal and Topog.) presents many forms produced by the denudation of different mountain structures. Daubuisson (Traité

The direction of the waves formed by the settling of any wide area will be parallel to the great synclinal axis; a fact already stated in different language by Professor Rogers. The present mountain ranges are the evidences of the ancient oceanic currents, and thus the causes which determined the elevations existed long before the production of the mountains themselves. At no point, says Mr. Hall, between the Appalachians and the Rocky Mountains, could a mountain chain have been produced, because the accumulated materials were insufficient. The Rocky Mountains owe their greater height to a series of later deposits than those which cap the Appalachians; the White Mountains are covered with a later formation than that which covers the Green Mountains; the Alps are newer than the Jura; and, generally, if it is the original deposition of the materials that has produced mountains, then the greater the accumulation the higher will be the chain when it is finished. The direction of later currents may not have coincided with that of earlier ones; and we may

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de Géognosie, vol. i. p. 230) describes Mont Meisner, in Hesse, six leagues south-east of Cassel, as an isolated mass of limestone and sandstone six hundred métres high, overlaid by a bed of sand containing fossil wood thirty métres deep, and the whole capped by a layer of basalt, from one hundred to one hundred and fifty métres deep: thus, he says, the top of this mountain, two thousand five hundred feet high, must have been originally dominated by volcanic mountains which have entirely disappeared. Mr. Jukes has considered denudation in its various forms, and urges its great influence in the production of the surface features of the globe (Address to the Geol. Sec. of the Brit. Assoc., 1862, pp. 54–64). He observes that all uptilted mountain chains have grown by slow degrees, the internal force thrusting up what the external agencies were always wearing down. Mr. Jukes, however, holds that no plications have been gained near to the surface; that all such disturbances have been acquired by "a slow, creeping movement;" and that the rocks thus disturbed ultimately reach the surface, because they have been laid bare by erosion: but, with such a conclusion we cannot agree, as we should have to consider true synclinal valleys to be valleys of erosion, which the facts do not confirm. Mr. Lyell (Manual, p. 71) says, that the Jurassic valleys are not due to aqueous excavation, but to those mechanical movements which have bent the rocks into their present form.

have diverging or cross ranges of mountains, with high summits, where the combined deposits from several sources have occurred: this effects the same result as the "chopped sea," of Mr. Rogers's geological tempest. This hypothesis explains the fact, that the higher the mountains are, the more recent are the strata which form their upper portions; and it also shows, that the notion that mountains have been extruded through fractures where the crust was weakest, is incorrect. Indeed, so far as the thickness of the sedimentary beds has any influence in strengthening the crust, the mountain lines are the strongest; and the weakest lines are to be found in the undisturbed regions where the beds are thin. It is believed by many geologists, that the interior of the globe is in a state of rest, which can be disturbed only by some external influence. Such, according to Mr. Hall, is supplied by large masses of sediment which load down the crust, and force it into the fluid matter, thus disturbing the equilibrium of temperature. Babbage and Herschel long ago suggested, that the deposition of large masses of sediment at any point would disturb the equilibrium of temperature, which might result in heating and expanding the rocks, and, according to Mr. Babbage, cause the rise of a chain of mountains, or even of continents. Thus, by the abrading of the surface-rocks, and the transport of the sediment to new places, we are supposed to have a means of exciting the fluid mass beneath to action.[1] We are, however, disposed upon this point to agree with Professor Hitchcock, that, if we have to descend fifty or one hundred miles to reach the supposed fluid matter, so very small a depression as would be made by placing any bed of sediment with which we are acquainted upon the solid crust, could have but little influence in altering the position of the isothermal surface.[1] Indeed, if any one will draw to a correct scale a portion of the earth's crust, and make it only twenty-five miles deep, and then draw a section of the Andes or the Himalayas as a load upon that crust, the idea of any settling from the weight of the mountains will appear perfectly absurd, even supposing the crust to have no support at all from the fluid matter beneath. Certainly, no body of sediments that we are acquainted with could exert any thing like the action of the moon, which Professor Thomson thinks would distort any globe with an average substance less rigid than steel.[2]

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[1] Herschel's letter to Lyell is in the Quart. Jour., 1836, vol. ii. p. 548; and the remarks of Mr. Babbage, upon the effect of an alteration of the position of the isothermal planes on the strata above, may be seen in the same volume, p. 73-76.

OBJECTION TO MR. HALL'S HYPOTHESIS.

In reply to an objection which might be made to Mr. Hall's hypothesis, — that the foldings made by subsidence would be removed by the final elevation, — Mr. Hall observes that the depression was local, but the subsequent elevation continental; that there is no more evidence of local elevation along the Appalachians, than in the basin of the Mississippi; that a large part of the strata which make this range still remains beneath the sea as a consequence of the great accumulation, while in the west a much larger proportion is above the sea. For a continental elevation, which Mr. Dana, we think, justly calls the most incredible of geological dogmas, neither Mr. Hall nor any one else offers an explanation; and we think it would be extremely difficult to do so: but, as Mr. Dana has suggested, an apparent elevation might be produced by the subsiding of the ocean beds, thus drawing the water off from the land; and such might well be, as the oceans occupy two-thirds of the area of the globe. But, supposing it possible that the whole continent could be elevated bodily, such a movement would by no means account for the existence of mountains: it does not satisfy the conditions which the present state of affairs presents. If, as Mr. Hall says, the folding and plication have added nothing to the height, and if no local elevation has occurred, why are the Appalachian summits now above the Mississippi basin, the Rocky Mountains above the great western desert, the Alps above the plains of Lombardy, or the Andes above the Pampas of South America? If there has been no local elevation, the relief of the continent must have been the same as it now is, denudation excepted, when the Appalachians were receiving their latest deposits in a shallow sea; in which case Illinois would have been submerged four or five thousand feet during the carboniferous age. The height of the mountains, Mr. Hall says, is not due to the folding or plication of the beds; the cause producing the disturbance is not that producing the elevation, the accumulation of sediments has been local, but the elevation continental: and yet he says that the line of the great Appalachian barrier is due to the original deposition of materials; that the present mountain ranges are evidences of the ancient oceanic currents, and the causes that determined the elevation existed long before the production of the mountains; that no mountains could have been made between the Appalachians and the great western ranges, because the material was insufficient. Now, does it not follow, that if the accumulations are local, the foldings dependent on the accumulations, the mountains present just where the foldings are most extreme and accumulations deepest, and not where the strata are horizontal and of but little depth, that the elevation is also local, and not continental; and that the folding and elevation, if not in some way to be regarded as cause and effect (though we may not see the connection), are yet to be considered as proceeding from a common cause, or, at any rate, as forming parts of a common process? We think with Mr. Dana upon this point, that, when the last of the sedimentary beds is laid down, the whole is still under water; and that some force is needed to raise them above the ocean to entitle them to a place among the earth's mountains.

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[1] Manual, 1859, p. 321.

[2] Mr. Lyell reminds us (Manual, Am. ed. 1861, p. 68), that, as all deposits come *from* some place, any increase in one locality must be balanced by a decrease to the same amount somewhere else. Now, if such an increase causes a subsidence, would not the decrease allow a corresponding elevation in some other quarter?

MR. HUNT'S IDEAS, AND OBJECTIONS THERETO.

Mr. T. S. Hunt[1] remarks, that the error of prevailing ideas upon the nature of mountains may be traced to the notion that the disturbed condition of the strata is not only essential to mountain structure, but evidence of its having been formed by local upheaval. He says that 'the upheaval has, in all cases, been continental, and not local; that disturbance is neither dependent upon elevation, nor essential to the formation of a mountain; that the accumulation of sediments would cause the flexible crust to subside; that the lower strata, altered by the ascending heat of the nucleus, would crystallize and contract; that plications would thus be made parallel to the line of deposition; that such plications, together with the softening of the bottom strata, establish lines of weakness in the earth's crust, and thus determine the contraction which results from the cooling of the globe to exhibit itself along those lines. Thus, Mr. Hunt considers the subsidence invoked by Mr. Hall, though not the sole or principal cause of the corrugations of the strata, as the one which fixes their direction.

If the elevation could take place from the expansion of submerged masses, as supposed by Mr. Babbage, we should suppose that it would be *local*, as only that part would be expanded which was depressed; and while Mr. Hunt's idea, that the ascending heat would cause the sinking crust to *contract*, seems decidedly opposed to what we know of heat, and to the expansion assumed in the hypothesis of Mr. Babbage, the idea that the Appalachian foldings could be made by such contraction, added to that dependent upon the general refrigeration of the globe, we think is open to all the objections already urged to the theory of Mr. Dana. Finally, while Mr. Hunt holds to continental and not local elevations, he says distinctly, that the general contraction of the globe has been controlled *by local causes;* that the lines of sedimentary deposition have fixed the *places* where the more general causes should operate.

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[1] Am. Jour., 1861, vol. xxxi. p. 409. Paper on the recent progress of American Geology.

THE ORIGIN OF SEDIMENTARY ROCKS.

A wide difference of opinion exists among geologists, as to whence the material for the sedimentary beds was derived. In the case of the Appalachians, Mr. Rogers concluded that a large continent formerly existed, south-east of our present land.[1] The evidence of this is found in the fact that the conglomerates and coarse sandstones appear in great force, chiefly towards the south-eastern portion of the Appalachians, and thin out in going west, being finally replaced by the fine-grained sandstones, shales, and limestones, — all showing that the current was strong, the water shallow, and the material near to the south-east; while, west of the Appalachians, the finer sediments were carried to greater distances, and laid down in a tranquil sea. Mr. Lyell expresses the same idea. Mr. Hall does not refer particularly to this matter, but speaks of the Appalachians and other mountain ranges as marking the position of ancient oceanic currents; from which we should infer that the current flowed from Vermont to Tennessee. Mr. Dana differs entirely from Mr. Rogers as to the existence of former continents, and as to the source of the sediments. In his several papers already referred to, he gives us the most elaborate history of the various phases through which our continent has passed, that is anywhere to be found. It is impossible to do him justice by presenting the exceedingly brief abstract which serves our purpose here: we can only refer the reader to the original papers in the "American Journal," or to the "Historical" portion of his "Manual of Geology."

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[1] Trans. Assoc. Am. Geol. and Nat., 1840–42, p. 433 *et seq.*

According to Mr. Dana, the first land which appeared above the ocean was a tract extending from Labrador south-west to Lake Huron, and from Lake Superior north-west to the Arctic Ocean. This little continent was shaped like a V, the two arms enclosing Hudson's Bay; the longer or western arm is parallel to the Rocky Mountains and to the Pacific coast, while the shorter or eastern arm is parallel to the Appalachians and to the Atlantic coast. Thus, the very germ of the continent shows the action of the two principal forces, which continued to act until the Appalachians and Rocky Mountains took their places as the border features of the continent. From this commencement the land gradually expanded, area by area southward; each successive formation having its margin against the preceding one. The force from the south-east in the very dawn of Paleozoic time, made the Appalachian region shallower than the basin within; the water increased in depth from Canada. south-west; a strong current flowed from Labrador along the course of the Appalachians, while another took the direction of the Rocky Mountains, and gradually a larger V began to develop outside of the former one. These two V's were separated by two great valleys; the longer or western arms by the Mississippi and Missouri, and the smaller or eastern ones by the St. Lawrence: thus, long before there were any mountains (at least above the sea), there existed a great bar which cut off the continental basin from the Atlantic Ocean on the east, and another one which separated it from the Pacific on the west. The character of the rocks from Vermont to Tennessee shows that the movements were such as both to elevate and depress the region occupied by the Appalachians; but, while this zone was thus oscillating, and receiving immense deposits of coarser and finer matter, the Mississippi basin underwent little or no change, as shown by the regular thin beds of limestone. Whatever doubt there may be as to the cause of these V's, there can be none as to the facts. The Appalachians were made by a long acting, immense force, exerted towards the north-west; a force so gradual that the strata could bend without being reduced to chaos. There is no proof in the flexures of the strata of any emergence during their formation; yet, when the revolution ceased, the mountains were left as they now stand. No facts, says Mr. Dana, show that a continent now sunk ever existed, from which the sand and gravel for ours were obtained: the whole system of geological progress is opposed to it. The great oceanic currents are, in general, too feeble to transport any material coarser than fine sand. In past geological ages, the rapidity of these currents must have been less than now, if there was any difference, because of the less difference then between the temperature of the equator and the poles. The action of the sea is mainly confined to the space between high and low water: the waves near the coast beat back the detritus thrown out by rivers, and so cause it to be deposited against the shore. No continent can contribute to the detrital accumulations of another one, except through the aid of icebergs; the detritus dropped by icebergs is a confused mass, and not a regularly stratified bed; no conglomerates or coarse sandstones can be made from the deposits of oceanic currents, but only fine-grained rocks. Tides and waves all work shoreward; and, if a continent ever existed in the North Atlantic, America would have received from it little or no material. The Azoic age left the surface of the earth more or less covered with sand and gravel, both above and below the water; and this material was worked over and over again, to make the sedimentary rocks: igneous and chemical operations have added but an inconsiderable amount to the mass of the formations.[1]

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[1] The Azoic age is that period when the first land, above referred to, was being formed.

Mr. Dana's opinions, as above expressed, may be seen in Am. Jour., 1856, vol. xxii., pp. 305 and 335; and in his Manual, pp. 655-660.

We endeavor, throughout this paper, to give the exact ideas of the several authorities referred to: but it is seldom practicable to give the precise language of the writers; as the material from which we draw is in the original generally developed at considerable length, Professor Agassiz would also seem to deny the existence of an early continent, from which the material for ours was obtained, since he says that America was the first land washed by the ocean which enveloped all else beside.[1]

DIFFICULTY OF ACCOUNTING FOR THE SEDIMENTARY ROCKS.

It is difficult, after all, to understand where the material for the sedimentary rocks, when we consider the vast amount of it, *did* come from. The production of a mass of deposits fifteen hundred miles long, one hundred and fifty miles wide, and in some places forty thousand feet deep, is inconceivable from analogy with any thing now going on. Even if we admit, with Mr. Rogers, that an ancient continent existed south-east of ours, and we suppose other continents for the Rocky Mountains, Alps, Andes, and Himalayas (a solution which only avoids one question by presenting another equally hard to answer, for we still have the first continent to account for), we are aware of no means by which the immense beds of conglomerates and of coarse sandstones could be transferred from one continent to the other. Nor does Mr. Dana help us much in this matter: for all the source which he offers for these immense beds of sediment is that small Azoic land, at first referred to; *i. e.* the wash from a small continent can make a large one: and, while he states that an ocean current cannot deposit conglomerates, he admits the existence of immense beds of such, hundreds of miles from the only known source. The simple working over by water of a submerged surface could not produce any sequence of formations: even if such a process could make a thin bed of detritus, that very bed would protect the underlying rock

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which, though best for the purpose for which it was prepared, is not suited for a brief review such as this. When the exact language of an author serves our purpose best, we have not hesitated to use it: thus, in the remarks of Messrs. Rogers, Dana, and Hall, we have, in some cases, for a few lines, used their language as well as ideas.

[1] Atlantic Monthly, 1863, vol. xi. p. 373.

from further wearing. Mr. Dana himself admits that such an action would probably be much less than one hundred feet in depth.[1]

The facts recently brought to light by the Canada Survey are important in this connection, though it must be confessed, that, instead of clearing up any of our former difficulties, they seem rather to increase them. Before proceeding to the results of the studies of Sir William Logan and his assistants, we must look for a moment at the common notions regarding the fundamental rocks. In the ordinary teaching of geology, we are given to understand that the foundation-rock of the globe is granite, and that the fluid matter supposed to underlie the solid crust is melted granite; that the difference between the matter ejected from volcanoes and the fundamental rock lies in the mode of cooling: the former, solidifying in the open air, becomes lava, basalt, or the like; while the latter, slowly cooled under enormous pressure, becomes the mineral compound called granite. When this appears at the surface, it does so not because it was formed there, but because some superficial covering has been stripped off from it. When the overlying rocks have not been removed, we pass in descending through various alternations of limestones, sandstones, slates, shales, and conglomerates, containing more or less the remains of the animals which lived in the seas where these now solid rocks were once laid down as incoherent masses of sediment. As we pass downward in the series, the buried animals become further and further removed in their physiological characteristics from existing species, until in the lowest Silurian beds we seem to have arrived at the beginning of animal life: so, at least, it has been considered by Agassiz, and other eminent naturalists. Between these lowest sediments and the fundamental granite, we are told that there exists a series of rocks, sometimes of very great thickness, which seem to belong neither to the crystalline nor to the sedimentary rock entirely, but to partake somewhat of the nature of both, since they bear plain traces of what are supposed to have been layers of deposition; but the materials which form these layers are well crystallized, and show as good specimens of mica, quartz, and felspar, as are found in granite: moreover, there is every possible gradation, from sedimentary rocks containing fossils, to the most homogeneous granite,—in some cases by such slow degrees, that we can hardly draw the line separating the lower from the intermediate, or the intermediate from the upper. The older geologists called these the transition series, as they were supposed to show a transition in the operations which produced the extreme varieties of the rocks. No signs of life being detected in these "schists," it was supposed either that they were made as they now appear, and preceded the creation of life, or that they were formed as common sediments, and afterwards so baked and altered by heat that the fossils were obliterated, while the change was not enough to destroy all traces of bedding. The Canada Survey, however, has brought to light facts which are entirely opposed to all such doctrines. There had long been recognized in the Hebrides, in Western Scotland, and in Norway, vast beds of plicated and contorted crystalline schists of a different nature from the ordinary schistose rocks, and which were found to be inferior in position, even to granite itself: these were called by Murchison, "Fundamental Gneiss," and by Naumann and Keilhau, "Primitive Gneiss and Primitive Slate" (*Urgneiss* and *Urschiefer*). But it was left for Mr. Logan and his assistants in Canada to make us acquainted with the real nature and vast extent of this (at present) most ancient known formation. The tract described above by Mr. Dana as the earliest American continent, and which reaches from Labrador through British America, and called by him "Azoic," since the rocks were supposed to show no signs of life, is composed of formations, called by the Canadian geologists "Laurentian" and "Huronian," occurring as they do along the St. Lawrence and Lake Huron. This vast tract, the largest one anywhere known of these ancient rocks, consists, according to Mr. Logan, of some forty thousand feet in depth, of gneiss, schists, conglomerates, and limestones,[1] showing beyond possibility of doubt their sedimentary origin, and in some cases even containing organic remains. Now, according to Professor Dana, this tract of Laurentian rocks was the first land above the ocean; and yet that primal continent, in the structure of its rocks, points back to a period so remote, that the mind is unable to penetrate to that ancient time; points back to an age when these Laurentian sediments were worn away from a still pre-existing land, and, as if these rocks were especially intended to overwhelm the understanding, these ancient conglomerates contain *stratified* pebbles, which carry us yet further back in this boundless chronology.

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[1] Manual, p. 665.

Professor Mather, in a paper on the Physical Geology of the United States East of the Rocky Mountains (Am. Jour., 1845, vol. xlix. pp. 1–20, and 284–301), speculates on the causes and directions of the ancient oceanic currents, and of their influence upon the sedimentary deposits of the United States, and proceeds thus: The earth is a cooling body; cooling bodies diminish in volume; contracted bodies revolve with augmented velocity; increased velocity induces a greater centrifugal force, and this force a greater oblateness of the sphere; increased oblateness causes a flow of water towards the equator, and this flow would be deflected to the west as it approached the tropics by the rotation of the globe from west to east; atmospheric actions would modify these currents; the relief of the country, while most of the land was beneath the sea, was mainly the same as now, and the land was elevated in mass. The equitorial current flowing west over Brazil was deflected by the Andes, through the Gulf of Mexico and Mississippi valley, by the great lakes and St. Lawrence, and then was turned south-south-east by the polar current down the Champlain and Hudson valleys, and finally south and south-west by the Green Mountains and New York Highlands. This current brought tropic plants from the south for the Pennsylvania coal-fields; while, from the fact that the Arctic lands are barren and rocky, it is inferred that they have been worn away to supply the coarse, non-calcareous sediments, for the Appalachians. The folded strata and steeper north-west dips, where the strata have "fallen over in a westward direction," he explains by supposing a mountain to be suddenly elevated to a mile in height, when the *inertia* would cause the mass to press over to the westward; *i.e.* the mountain would be tossed up into the air, and the earth jerked out from under it. In the case of the double system of inversion of the Alps, we suppose some paroxysm jerked the earth first one way and then the other, as in this case ordinary rotation would be ineffectual. Mr. Dana, referring to Professor Mather's paper, observes that he has presented his ideas with much *force;* with which opinion we entirely concur. Geologists, who resort to such forcible dynamics, may well believe that the world was created in a week, literally.

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[1] The term "Azoic limestone" is somewhat contradictory, if, as many geologists teach, no extensive limestone beds are formed except by organic agencies.

According to Mr. Dana, these Azoic or Laurentian rocks are the oldest and the only universal formation upon the globe that we have knowledge of, underlying everywhere every thing else:[1] they include all beneath the Potsdam sandstone (Lower Silurian). These beds, which are metamorphosed and folded into sharp plications, have a remarkably uniform direction from north-east to south-west; thus generally parallel to the course of the Appalachians and to the Atlantic coast. The Azoics in Scandinavia have also a north-east run; but this should not be considered as necessarily connecting them with the American Azoics, since the same magnetic bearings at different parts of a sphere are not parallel; a north-east course in Asia being at right angles to the same course in America. But, in both America and Scandinavia, the general run of the Azoics will be found approximately parallel to the main lines of mountain elevation and disturbance, and coast lines. The original crust, says Mr. Dana, is now so disguised by, or buried beneath, the Azoics, that geology can hardly expect to identify any portion of it.[2]

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[1] The Azoic rocks, says Mr. Dana, were made out of the ruins of older Azoics; *i.e.* of the sands, clays, and stones gathered and deposited by the ocean as it washed over the earliest formed crust of the globe: whenever the ocean took its place upon the cooling globe, the conflict began, and sedimentary beds were the result (Manual, p. 143). It is difficult to understand how an ocean, working over a submerged surface, could make forty thousand feet of sediments, if such sediments are a universal formation, and if marine currents produce no effect except between high and low water, and are not able to transport any thing but fine sand.

[2] Professor Naumann (*Lehrbuch der Geogonosie*, vol. ii. p. 7), speaking of the primal rocks, observes, that there must have been, before the

MR. BILLINGS'S OBJECTION TO MR. HALL'S PLAN.

The objection has been urged[1] against the plan proposed by Mr. Hall to account for the folding of the Appalachian strata, that the lateral compression necessary to produce four or five such waves as existed before denudation in that region

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oldest sedimentary formation, a material from which the sediments were derived, upon which they could repose, and which itself could not have been sedimentary; also that the eruptive rocks must have been erupted *through* something, and have had some foundation whereon to rest, which, in itself, was not eruptive. And thus, he says, we find ourselves led in both directions to an original solid crust, which served as a basis for all subsequent formations, above which the sedimentary rocks were formed, and from beneath which the eruptive rocks came. To such primal formation, he observes, would belong those deep-seated masses, which, by their more or less perfect stratification, belong to the sedimentary rocks, and by their mineral condition and crystalline structure to the eruptive: it would lack the organic remains of the higher, and the injected masses of the lower rocks. Professor Naumann objects to the idea that such schists are formed from sediments, because this would infer no beginning; and this primitive formation has a very great depth and penetrates deep into the earth, and, wherever found, shows very uniform and peculiar features which suggest a world-wide cause: but, while admitting (vol. ii. p. 9), that the similarity between the primitive and newer gneisses would seem to indicate a similar origin, he afterwards (vol. ii. p. 154), thinks that we may as well assume a doubtful origin as a doubtful metamorphosis, and suggests that these schists were made as they are now. The results of the Canada Survey, however, leave no room for doubt upon this point, at any rate as far as concerns the Laurentians.

"Die Nothwendigkeit" (says Naumann, vol. ii. p. 8) "einer primitiven Formation ist übrigens so einleuchtend, dass man kaum begreift, wie sie jemals bezweifelt werden konnte. Sie erscheint in der That als ein unerlässliches Postulat, ohne welches die Möglichkeit sowohl der sedimentären als auch der eruptiven Bildungen gar nicht zu begreifen ist."

"Worauf" (asks Humboldt, Cosmos, vol. i. p. 299), "ruhte das älteste Sedimentgebilde wenn Gneiss und Glimmerschiefer nur als umgewandelte sedimentschichten betrachtet werden müssen?"

Mr. Thomas MacFarlane, in vols. vii. and viii. of the Canadian Naturalist, 1863, compares the primitive rocks of Scandinavia and of

[1] By Mr. Billings, in Canadian Naturalist, 1860, vol. v. p. 409.

would amount to upwards of four miles; while the terrestrial arc seventy miles, or one degree long, would, in subsiding to its chord, be compressed only six feet, or enough to make one wave, say a foot high. This objection, however, we think misapprehends the operation to which Mr. Hall would refer the making of mountains. The very little compression which would result from forcing the terrestrial arc into its chord, say six feet in seventy miles, is certainly no cause for folding of the strata. Such shortening would most probably take place in the most rigid rock by a yielding of the material; indeed, the phenomena of cleavage, soon to be noticed, show that a much greater compression does, in some cases, occur. If we

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Canada, and accounts for them as follows: The primitive gneiss, which he considers the equivalent of the Laurentian, is the first solidified crust of the originally fused globe: if the central fluid now forms unstratified rocks, it would have done so then; currents working in this semi-fluid igneous crust would produce a schistose structure, as we see done in pasty slags; such a crust would, after a while, yield to the terrestrial contraction, and fall in; and, by lateral squeezing, would get the fragments into the position in which we now find them (*i.e.* generally folded up at a high angle). This state of affairs he compares to the ice-jams on the St. Lawrence. That a pasty mass of semi-fluid rock would be drawn by the supposed currents into the laminæ, and even strata of different mineral matter, we do not believe. What effect the tidal wave would have in this first crust, we cannot say; but, from Professor Thomson's conclusions, we should suppose that it would be a very marked one. The crust might, if rigid, collapse by contraction; but we do not think that the shortening by contraction would be enough to account for the numerous folds seen in the Azoic schists. The ice-jams in the St. Lawrence are immense *accumulations*. Mr. MacFarlane thus does not consider the Laurentians as sedimentary; but to the Huronians, which overlie the former, he allows such an origin. The Canada Survey, however, teaches that the oldest Laurentians are metamorphosed beds of sedimentary rocks, and not originally crystalline; indeed, Mr. Hunt remarks that we see in the Laurentians evidence of life as varied and abundant as that of Paleozoic times. — See also Logan, Dawson, and Hunt on organic forms in the Laurentian rocks, in Quart. Jour. Geol. Soc., 1864, vol. xxi. pp. 45, 51, and 67; also Bigsby, on the organic contents of the older metamorphic rocks, in Edin. Phil. Jour., 1863, vol. xvii. p. 171.

understand it, the shortening which results in the compression required, arises from the forcing down by gravity of a great mass of sediments, the lower part of which would subside to a depth equal to the whole thickness of the stratified rocks; which, in the case of the Appalachians, would, in some parts, be over six miles. According to Mr. Dana, the region of the Appalachians was a sort of submarine ridge at the earliest Paleozoic period, on the top of which, sediments were laid down in a much larger quantity than anywhere else east of the Rocky Mountains. This ridge must have subsided to an extent equal to the whole depth of the sedimentary formations, from the bottom of the Potsdam sandstone to the top of the carboniferous. What, now, would be the condition of the sediments laid down, layer by layer, upon this slowly sinking ridge, the supply of detritus being always kept up?

The deposit being greatest along the central line, that part will settle with the greatest energy, and will thus be subjected to an immense compression, which will be greatest where the depression is greatest, and will decrease in going towards the margin of the district where the accumulation thins out. The deeper the mass of sediments, the greater will be the compression, and consequently the more extreme the plications; and we actually find the higher mountains, denudation of course being allowed for, as a general thing more disturbed than lower ones. Indeed, we may safely conclude, that the existence of plications or foldings is positive evidence of compression, and of nothing else: we believe this may be taken for a general axiom in geology. Studer's "Geologie der Schwyz" is full of evidence of the enormous pressure that has folded the Alpine and Jurassic strata, in many cases even jamming the waves up into parallel vertical beds, of which the section from Chamouni to Courmayeur is a good example. The Appalachian formations have been, by some geologists, considered to be generally conformable from the lower Silurian to the top of the carboniferous; but Mr. Hall does not so regard them;[1] and, if we suppose the foldings to be slowly made during the deposition and subsidence of the sediments, they would not be strictly conformable, nor would they be entirely unconformable: they would come as near to regularity as we find in nature, but not so near as the generalized sections we find in books. We are not obliged to consider the beds as entirely rigid, or as altogether incoherent during their formation and folding: they must have been soft enough to bend, though this may not be softer than some solid rocks;[2] and they must have been compact enough to remain distinctly separated beds, though this may not be harder than many unconsolidated sands and clays. We are at liberty to believe in any consistency, from time to time, that suits our convenience; and, of course, the density of masses of sediment would increase as the superincumbent weight, and therefore pressure, was augmenting: so that during accumulation a mass might pass from a loose to a compact state, under no influence except the action of weight applied above. Mr. Murchison says, in speaking of this matter, "It seems to me, that, however we may attempt to detect the power which produced the folds and contortions, we must admit that all the strata so folded together had been accumulated under the sea, and could have been only slightly consolidated before the operation commenced, by which they all partook of common and conformable movements of undulation. I believe such flexures were among the earliest of the great physical changes impressed upon these submarine strata, which, at the time they were so bent, may have been no more compact than many soft deposits we find in Russia."[1] So far as we have examined the facts, no other hypothesis than that of the slow sinking of vast masses of yielding sediments can at all satisfactorily account for the plication and other evident effects of a compressive force so invariably exhibited in mountain districts; and, however little we may understand the precise nature of such an operation, it seems confirmed by the remarks of La Bêche regarding the Alps, already referred to, which, he says, seem to show that the component beds have been squeezed up from both sides against a main central line corresponding with the main range of mountains (which would be Mr. Hall's line of greatest accumulation), thus throwing the beds into flexures very abrupt at the centre line, but decreasing in going towards the margin.[2] Professor Guyot considers the Alps and the Jura to have been produced by lateral pressure, and to have so re-acted against each other as to pucker up the newest beds in the plains of Switzerland. He also observes, that, while generally the Jurassic slopes are most abrupt towards the Alps, the arches of the chain nearest the Alps are often pushed over so as to make the northern side steepest, which also presents a crushed look; and that the disturbances of the rocks, both in the Jura and in the Swiss vale, increase in abruptness as the Alps are approached.[3] Saussure, and later Studer and Naumann, also teach that the Alps and the Jura are, in some way, connected in their origin.[4]

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[1] Mr. Hall adduces the unconformability of the Hudson and lower Helderberg groups at the Catskill Mountains as evidence of a periodical subsidence. We can hardly see how the whole series *could be* conformable, since the lower Silurian sunk half a dozen miles while the superincumbent beds were being deposited, and must therefore have been undergoing folding during the deposition of the later beds, which must have been affected by such disturbance in the surface on which they were laid down.

[2] There is scarcely any material, says Mr. Dana (Manual, p. 721), so solid, that, when in broad tabular masses, it will not become flexed under a gradual heavy pressure; but he thinks that, generally, the arenaceous and argillaceous beds were not firmly consolidated, but derived their consolidation from the heat which escaped from below during the progress of the movement, and which was the cause of the metamorphism where the plications were most numerous. We shall see hereafter, that such heat may be derived from another source than that supposed to exist below.

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[1] Quart. Jour. 1849, vol. v. p. 239: paper on the Alps.

[2] Same vol., Annual Address to Geol. Soc.

[3] Proc. Am. Assoc. Adv. Sci., 1849, pp. 115, 116.

[4] Saussure, Voyages, § 330. Studer, Geol. der Schwyz, vol. ii. pp. 209, 210; and Naumann, Lehrbuch, vol. i. p. 949.

There may, perhaps, be some parallelism between the relation of

There is another matter, which, if it needs explaining at all, Mr. Hall's hypothesis seems to account for in the only conceivable manner. La Bêche suggests, that, if a mass of beds were crumpled up, thus leaving a wide rent in the crust, something would be necessary to fill up the space thus left open by the shortening of the superficial extent of the strata. If the foldings were made by the sudden application of any force pushing the rigid beds laterally, we might suppose that something would be needed to hold the folded strata in place: but, if the compressing and folding resulted from the gradual settling of yielding masses towards a central line by their own gravity, such filling-up might not be needed at all; or, if it was, would be furnished by the constant supply of sediments from above, as it was wanted. Mr. Jukes observes,[1] that all contortion and plication of strata has been acquired by a "slow, creeping movement;" and we shall see, hereafter, abundant evidence that such has been the case. It seems very strange, that many geologists, while admitting that the strata have subsided to a depth at least equal to their thickness, and that they have been subsequently elevated, either locally or generally, attribute the folding of the rocks, not to the downward, but to the upward movement; when, as Mr. Rogers observes, an upward movement would extend and not compress the surface; and evidently no folding could be made by extension.

Thus far we have confined our attention to the examination on the large scale of the several plans proposed, to account for the origin of mountains. It will be necessary now to look more in detail at some points which have a close connection with the subject, and which call upon chemistry and physics to bring their aid to the examination of geological phenomena.

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the Alps and the Jura, and that between the Appalachian system and the Ohio axis of elevation.

[1] Proc. Brit. Assoc. 1862. Address to the Geological Section.

THE METAMORPHIC ROCKS.

We have already referred to a class of rocks which appear to have had a double origin; which seem to have been produced as one rock, and afterwards by some means to have been converted into a different one. These rocks, the transition of old geologists and the metamorphic of later ones, are so closely connected with tracts of disturbed strata, otherwise with mountain regions, that we cannot avoid concluding that the mineral character of the metamorphic rocks and the folding of the strata result from some common cause; and, upon the nature of this cause, the examination of the intimate structure of the metamorphic rocks may throw some light.[1]

It may not be possible to say exactly what it is that constitutes metamorphism. The change is so gradual from the least to the most altered rocks, that we can hardly draw the line at any point, and say that *there* metamorphism commences. Professor Hitchcock observes, that there is probably no rock with which we are acquainted that now possesses its original character. It is not common to consider simple solidification of a mass of sediment as a metamorphic operation; though it may be found that this change is the same in kind, but only less in degree than that producing the more extreme changes which result in entire obliteration of the original structure, and the establishment of a new one. Confirming this idea, we actually find in nature every possible gradation, from incoherent beds of sand and clay, to the most crystalline and well-developed granite; and we might arrange a suit of specimens from one to the other, so that, from inspection of any one, no person could tell whether it was nearest allied to the one above, or the one below it. Let us very briefly examine the nature of the principal rocks with which we meet, those which occur in wide-extended masses, and would form such a series as above mentioned; and, afterwards, as briefly look at the several hypotheses which have been proposed to account for the mineral changes called metamorphic.

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[1] "Metamorphic rocks are always displaced and folded rocks, and never for any considerable distance horizontal. Where the foldings are most numerous and abrupt, there the metamorphism is most complete. In the case of mineral coal, the bitumen is more completely expelled, the greater the disturbance of the strata." — *Dana's Manual*, p. 710.

"It is only in the regions of great accumulation of transported sedimentary matter" (previously shown to be that of mountain zones), "that the conditions of metamorphism exist, or, at least, operate on an extensive scale." — *Hall's Introduction*, Note D, p. 90.

"As a general rule, the signs of metamorphism and disturbance are met together, and in similar proportions. The greater amount of metamorphism is mainly due to the greater energy of dynamical causes." — *Scrope's Volcanoes*, ed. 1862, p. 298.

"Regions of altered strata appear to be in all cases folded and corrugated." — *Canada Survey*, p. 585.

"L'origine de la structure feuilletée, de même que l'état métamorphique, parait se lier essentiellement à l'existence de dislocations." — Daubrée, *Études sur le Métamorphisme*, p. 67.

Archiac, in the introduction to his vol. for 1853 (Hist. Géol., p. 4), says, "On voit d'après cela, que si les caractères pétrographiques d'une roche ne sont plus aujourd'hui pour le géologue un indice de son ancienneté relative, ils sont cependant encore un signe assez certain du plus ou moins d'énergie des phénomènes dynamiques, qui ont accidenté le système de couches auquel elle appartient; car l'importance des modifications qu'elle a éprouvées, indépendamment de là proximité des produits ignés, est en raison du plus ou moins d'intensité de ces mêmes phénomenès."

Granite is a compound of quartz, felspar, and mica, confusedly mixed; and in which the several minerals have, in their formation, so interfered with each other, as to prevent the formation of perfect crystals. It has generally been considered as the product of a molten mass, which has cooled very slowly, and under great pressure; as precisely the same ingredients cooled rapidly and not under pressure, do not form granite, but a compact rock of a uniform texture. True granite, though often divided by more or less regular joints, shows nothing which we can call structure; but it passes into a rock of precisely the same mineral constituents, which shows a laminated or bedded arrangement: this is gneiss, — the stratified granite of some geologists, — in which the felspar, quartz, and mica are more or less definitely arranged into irregular layers of the different minerals, a section at right angles to such layers showing alternate bands of white and black; the former being quartz and felspar, and the latter chiefly mica. The varieties of gneiss are very numerous, according as the several constituents bear different relations to the whole mass, and as other minerals, hornblend, talc, &c., enter into its composition. Next to gneiss comes one of the most abundant of the so-called metamorphic rocks, and into which gneiss passes by imperceptible gradations,—mica schist; a more micaceous and slaty, less compact and crystalline rock; sometimes, indeed, consisting almost entirely of mica. Chlorite and talc again may enter so largely into this as to form chloritic or talcose schist. Finally, mica schist passes into the fine-grained fissile rock, called argillaceous or clay slate, to which our roofing and writing-slates belong. Still lower in the series is shale, which is simply a slightly consolidated, laminated mud. Sedimentary deposits of sand again may take any form, from strata altogether unconsolidated, to that soft "freestone" which is sawed into slabs at the quarry; and still farther on to the compact sandstones, until we come to quartz rock, or quartzite, a highly indurated sandstone, composed of grains of quartz, which sometimes appear as if fused together at the edges, and sometimes as if embedded in a siliceous cement. Sandstone will be siliceous, argillaceous, or calcareous, according to the nature of the sand-grains, though the latter is generally called sand-rock. Somewhat allied to this class is that of the grits and conglomerates; the former composed of sand and small pebbles, and the latter of pebbles of all sizes, cemented together by calcareous, siliceous, or other matrix.[1] Sandstones, by the addition of mica, pass into micaceous sandstones, and finally into mica schist.

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[1] "A mechanical rock," says Mr. Sedgwick, "may appear highly crystalline, because it is composed of crystalline parts derived from some pre-existing crystalline rock. Thus we have, in Scotland, masses of sandstone so recomposed from granite, that it is not always possible to distinguish them from the parent rock."—*Sedgwick on "Large Mineral Masses," Geol. Trans.*, 1835, vol. iii. p. 479.

These numerous varieties will occur in all conceivable proportions and alternations, and the same bed will often change its mineral character as it is followed *horizontally*; as might be supposed, when we remember the variations which are always taking place in the nature of the deposited sediments. The calcareous rocks may be followed from the soft, earthy limestones, made of the fragments of shells and corals more or less ground up by the action of the sea, through numerous members of the class, growing more and more compact, until we arrive at that crystalline variety called marble. The above rocks, in endless variety of induration and of color, and in every possible disposition, depth, and extent, form, so far as we are acquainted, the crust of the globe. It is in the study of these rocks in their various modifications, and especially in those transitions from one class to the other, which seem always to accompany disturbances of strata, that we are brought into close connection with the dynamical and correlated physical and chemical forces to which the orographical relief of the continents is due.

WERNER, HUTTON, AND LYELL, ON METAMORPHISM.

According to Werner,[1] at a remote period the different elements from whence rocks were derived were dissolved or suspended in a chaotic ocean, from which first the crystalline rocks, granite, &c., were deposited as a chemical precipitate. In the second stage, chemical precipitation went on, but in a less degree than before, while mechanical sediments were also laid down; so that the resulting rocks were of a mixed nature, chemical and mechanical: finally, the deposits were entirely mechanical, and produced the fossiliferous rocks, sandstones, limestones, and the like. During consolidation, ruptures took place, and produced the rents which were filled with aqueous incrustations, so giving rise to veins. Thus, all rocks were produced in the same condition as we see them now, and the universal agent was water.

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[1] A very complete reference to the numerous authors who have treated upon this department of geology may be seen in Daubrée's "Études sur le Métamorphisme."

We might ask of such an hypothesis, what the first crystalline deposit found to rest upon, and how to account for masses of granite sending veins into pure sedimentary rocks, when the granite was made first; and how it happens, that, in many cases, gneiss and mica schist lie above unaltered fossil-bearing sedimentary rocks. As it is, the Azoic Laurentian metamorphosed sediments, lying as they do beneath even the granites, undermine the very foundations of any such hypothesis.

According to Hutton, the lowest rocks were crystalline masses, formed by the cooling of a globe of molten matter. The upper surface of such rocks was first denuded by the atmosphere and the sea; the sediments thus produced were then deposited at the bottom of the ocean; and there, by heat ascending from below and from the pressure above, were first consolidated, next converted into a crystalline bedded rock; and finally so far heated as to fuse into granite, and to be re-absorbed into the interior, to be again upheaved, ready to repeat the same series of operations, to which there is neither beginning nor end. This hypothesis is the basis whereon Mr. Lyell founds his ideas regarding the metamorphic rocks, and to the support of it he has brought a great many facts and much careful study. He commences by illustrating the mineral analogies between the various metamorphic rocks, and the alterations that take place in the sedimentary beds where they adjoin masses which are supposed, at the time of their production, to have been in a melted condition. From the analogy between the so-called metamorphic rocks and those supposed to have been altered by heat, it is inferred that heat was the cause of the metamorphism; and no source was known for such heat but the supposed molten mass beneath the earth's crust. The accumulated sediments by their weight sink within reach of this heat, and, as with Hutton, become first schists, and next granite. This granite, with its overlying formations, may be afterwards elevated; and, by subsequent denudation of the surface, will expose to view either a metamorphic (schistose) rock, or granite, according to the depth which is stripped off from the surface.[1] Thus a mass of granite is not, as in Werner's hypothesis, necessarily a primitive mass, but may be of any age; as, from association with the various sedimentary rocks, we know to be the case. But we are left in the dark as to what the first rock in this perpetual revolution would be. To be sedimentary, it must, as before suggested by Naumann, have had some previous rock to rest upon, and to be made from; to be granitic, we have to admit that a molten mass will form a crystalline product at the surface, and thus without pressure, except such as possibly might be furnished by a very dense atmosphere. Such a plan does not account for the interstratifications of altered and unaltered rocks which we find; nor does Mr. Lyell's supposition, that a solid mass of gneiss a thousand feet deep and three miles long might be forced laterally in a solid state between the strata already existing, satisfy us upon this point, any more than his other plan, by which he first heaves all of the strata upon end; plunges, by some unaccountable mechanics, the lower ends into the heated regions; has the easily altered rocks metamorphosed, and the refractory ones left unchanged; and then again throws the whole mass back into a horizontal position.[2] Such desperate measures are not worthy of the distinguished author of the "Principles," nor at all in accordance with that system of gradual change which he has done so much to uphold. Mr. Jukes agrees with Mr. Lyell, that there can be nothing unphilosophical nor improbable in the idea, that the crust of the earth consists of materials passing through an endless cycle of mutations as above described. He also believes the foundation-rock, in all cases, to be granite.[3] To all such speculations the fundamental schists of Canada remain a standing protest; as they are not so far metamorphosed as many granites which overlie them, and which, from their position, cannot be regarded as having been erupted from beneath, but must have been formed where they are.[1]

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[1] Manual, chaps. xxxv. xxxvi. xxxvii.

[2] Manual, Am. ed. 1861, p. 614.

[3] Manual, 1862, pp. 172-307.

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[1] Mr. Dana attributes metamorphism to subterranean heat, acting in conjunction with moisture, and usually, if not always, under pressure: the amount of heat being less than that needed for fusion, though perhaps enough for plasticity; the pressure being not very great, and produced either by superincumbent waters, or by overlying rocks. The moisture he considers necessary, because dry rock is a non-conductor of heat (Manual, p. 707). The older schists, Daubrée thinks, in many countries have never been covered by a great depth of rock; but he refers to Humboldt's calculation, that the waters of the ocean would, if converted into vapor, press upon the surface with a weight equal to two hundred and fifty atmospheres (Études, p. 121). We have already (p. 52) referred to Professor Hitchcock's opinion, that there is little probability that detritus is conveyed to the bottom of the sea in sufficient quantities to cause an accumulation of internal heat such as would convert mechanical into crystalline rocks, notwithstanding the opinions of Babbage and Herschel, who were the first to bring forward these ideas. Professor Hitchcock thinks, however, that such heat would need to be nearly great enough for fusion of the rocks: but Mr. Dana observes, that from 300 to 1200° Fah. would be sufficient, and remarks that a low temperature acting gradually through an indefinite age would produce results that would not otherwise be brought about even by greater heat; and he says, that, if metamorphism was produced by the process supposed by Herschel, we should find horizontal or slightly inclined beds metamorphosed, which is not the case (Hitchcock's Manual, p. 321; Dana's Manual, pp. 707, 711). Mr. Hall observes, that we cannot suppose that the metamorphic rocks now visible have ever reached a depth of twenty thousand feet, or a temperature corresponding to such a depth (Introduction to Pal., p. 77). Mr. Hunt thinks that the presence of altered strata at the surface is evidence of great denudation, and that probably the gneissic rocks of lower Silurian age in North America were, when altered, overlaid by the whole Paleozoic series; and that the metamorphism of carboniferous strata in eastern New England points to the former existence of great deposits of newer and overlying beds, which were afterwards worn off. (Am. Jour. Sci., 1861, vol. xxxi. p. 413.) According to the same opinion, the Azoic or Laurentian schists must have been at some time deeply buried. An abstract of the very numerous plans of more or less value which have been proposed to account for the phenomena of metamorphism may be found in Naumann's Lehrbuch, vol. ii. pp. 148–156. — *Hypothesen über die Bildung des Gneisses.*

EXAMPLES OF METAMORPHISM AT TRAP AND BASALT CONTACTS.

As the common idea of metamorphosis by heat is founded upon the mineral analogy between the wide-extended masses called metamorphic, and those more local changes which seem to have been produced by contact with what are inferred to have been melted rocks, such as dykes and veins, it will be well for us to see what these latter changes are, and under what conditions they occur. Professor Bischof[1] describes at Meissner a layer of plastic clay from one to five feet thick, on top of which lies a large mass of basalt: the clay is divided into columns at right angles to the layer of basalt. In the Vogelsgebirge, near Ettinghausen, this columnar structure is seen for upwards of two feet and a half from the under side of the basalt: the columns of clay are of a reddish-brown at the contact, and for a distance of several inches; but, further off, the color is gray. Near St. Saturnine, in Puy de Dôme, a basaltic mass rests on clay containing numerous vegetable remains which seem to have been carbonized, while the clay is hardened and divided into small perpendicular prisms ten or twelve inches deep. At Habichtswald, the brown coal does not seem altered where the basaltic dykes are only from four to six feet thick; but, where they are ten or twelve feet thick, the coal is rendered columnar for one or two inches. Leonhardt saw at Wildenstein, near Büdingen, sandstone columns an inch in diameter, and over seven feet long, grayish-white at a basalt junction, while the unaltered sandstone was red.[2] The Cockfield-Fell dyke in the north of England, is from fifty to sixty feet wide, and seventy miles long. It cuts through a coal-bed six or eight feet thick. At one hundred and fifty feet from the dyke, the coal begins to look dull, and grows tender and short: as it comes nearer, it assumes the appearance of half-burnt cinder; still nearer, it becomes real cinder, and at the contact it resembles soot caked together.[1] In the north of Ireland, the greenstone and basalt dykes have changed the chalk through which they extend, into coarsely crystalline marble; soft greensand is changed into a hard, red grit-stone. A trap dyke, one hundred and thirty-four feet wide at Anglesea, passes through shale and argillaceous limestone, which it alters for thirty or thirty-five feet; the shale becoming harder and losing its schistose structure, and the limestone growing granular and crystalline: in the shale are found crystals of garnet, which are distinctly confined to the part of the rock affected by the dyke. Sedgwick also describes garnets in shale altered by basalt. Macculloch found in Skye sandstone changed to solid quartz and quartzite, by contact with masses of trap.[2] Mr. Phillips, in his paper on the Malvern Hills, before referred to, describes a trap dyke at Brockhill, on the Teme, thirty feet wide and passing vertically through horizontal beds of red sandstone and marl. On approaching the dyke, the beds grow less plain, and suddenly are lost. On one side, the rock is divided into prisms perpendicular to the plane of the dyke, and, at the same time, into vertical divisions parallel to the dyke: both the sandstone and the marl become hard, and changed in color.[3] Professor Dana describes a dyke of basalt eight feet wide in New South Wales, which cuts vertically through sandstone, coal, and clay: the coal for six or eight feet is deprived of its bitumen, but beyond that distance it is not altered; the clay is changed to a bluish flinty chert, for eighty yards from the dyke; the sandstones are also baked and hardened.[4] Professor H. D. Rogers[5] describes a dyke in New Jersey, which has baked the sandstone for a quarter of a mile; and from fifty to a hundred feet from the junction, the sandstone is filled with various crystalline matter, tourmaline, &c.

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[1] Chemical and Physical Geology, Cavendish Society's translation, vol. iii. p. 70–80. "Crystalline and Metamorphic Rocks."

[2] Bischof, vol. iii. p. 78.

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[1] Jukes's Manual, p. 329.

[2] Lyell's Manual, Am. ed., 1861, p. 480.

[3] Mem. Geol. Sur. Gt. Britain, vol. ii., Pt. I., pp. 156, 157.

[4] Am. Jour. Sci., 1843, vol. xlv. p. 115.

[5] Geol. of New Jersey, p. 149.

The preceding facts show plainly, that well-marked changes do occur at the contact with masses of trap and basalt: but Mr. Lyell observes, that, while in many cases shale becomes flinty slate, or jasper; limestone, marble; sandstone, quartzite; and coal, coke, — yet among the same rocks, and even in the same districts, and apparently under the same conditions, we often see no change at all; which he accounts for by supposing that the dykes may have had a different temperature at the time of their intrusion, and that the invaded masses had a different composition and structure.[1] He also thinks that the presence of water may have been necessary to convey the heat; an opinion to which Mr. Dana would assent, as he does not consider heat alone sufficient to explain the changes, and very justly remarks, that, to bake the rocks for a distance of twenty yards from a dyke by conduction alone, the first fifteen feet at least must have been in a state of complete fusion; while there are many cases where the rocks are altered to a greater distance than this, with no signs of fusion at the contact.[2] In the example above given by Mr. Dana, we should suppose, that, if simple heat had changed clay to chert for eighty yards, it would have done more than simply remove the bitumen from the coal for six or eight feet. The tourmalines, in Mr. Rogers's example above noticed, would seem to show that no great heat had been present in the alteration of the rocks. Strong reasons, says Daubrée,[3] make us believe that heat alone cannot have made the changes which we see, even had the temperature been high enough to soften the rocks, which in most cases is improbable, as it would not account for the diversity of the effects. The contact changes in the Christiania district, says Keilhau,[4] point to a much more complicated play of forces than the mere action of a melted mass on the bounding rock could have effected or produced; and he remarks, that the study of these phenomena leads most directly to a knowledge of the transforming processes going on in solid rocks, without the aid of an extraordinary degree of heat. Such processes, he says, have been especially active where heterogeneous masses meet, but that sometimes they present themselves at the contact of stratified rocks alone. There can be no question, says Bischof,[1] that the columnar structure of rocks in contact with basalt is evidence of the former high temperature of the basalt. He thinks that this structure is always the result of contraction, which action could only take place after a previous expansion, for which there is no assignable cause but heat. The change in color and chemical and mineral composition he attributes to the action of water, which finds access by means of the fissures produced by contraction: thus, heat is the first step that prepares the way for the water, which, entering with some minerals in solution, and producing others after it gets in by its action upon elements already present, is the second step, and thus the active agent. He also concludes that heat can in no way introduce silica for the silicification so often seen at contacts, but that such may easily be introduced by water. Delesse concluded, from analysis of contact products, that the change was made less by heat proper than by aqueous emanations charged with divers salts and acids.[2] Durocher also attributes metamorphism to slow actions, and to molecular operations similar to cementation.[3] We must look to some other agency than heat, says Mr. Hall, for the production of the phenomena witnessed (*i.e.* metamorphism); and he thinks "that the prime cause must have existed within the material itself, and that the entire change is due to motion or fermentation and pressure, aided by a moderate increase of temperature, producing chemical change."[4]

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[1] Manual, p. 482.

[2] Am. Jour., vol. xlv., p. 112.

[3] Études, p. 79.

[4] Phil. Jour., 1838, vol. xxv. p. 269.

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[1] Chem. and Phys. Geol., vol. iii. p. 79.

[2] Études sur le Métamorphisme, Ann. des Mines, 5 série, 1857-58; t. xii.-xiii.

[3] Études sur le Métamorphisme, Bull. Soc. Géol., 1846, t. iii. p. 547.

[4] Introduction to Pal., p. 78.

Bischof repeatedly heated to redness fragments of unaltered sandstone, with no change except increasing the depth of its color. Pro-

Heat, says Daubrée, cannot explain the presence of andalusite and staurotide (silicates of alumina) in fossiliferous clay slate, or felspar and pyroxene in unaltered limestones. The conjunction of heat, water, and pressure, however, he considers sufficient to account for the various phenomena of metamorphism;[1] and the experiments which he made fully sustain such an opinion.

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fessor Hausmann, of Göttingen (Bemerkungen über die Benutzung metallurgischer Erfahrungen bei geologischen Forschungen, 1816; and Edin. Phil. Jour. 1837-38, xxiii. pp. 326-30; xxiv. p. 65-85) says, "The heat of the smelting furnace often produces in stones, and other masses contained in their centre, lasting changes, having a perfect analogy to those displayed in mountain rocks, which, by some means or other, have come in contact with masses, to which an igneous origin is ascribed." Clay-slate, at a blast furnace at Magdesprung, from long operating heat, without becoming fluid, acquired an aspect like that of siliceous slate; and the sandstone walls of a furnace on the Hartz were completely transformed by the penetration of the silicate of the protoxide of iron. In Hausmann's illustrations, the silicates are furnished by the melted slags of the furnace, which would not be present in simple dry heat.

[1] Études, p. 80.

Daubrée produced perfectly clear, double pyramids of the form of quartz, from glass heated in the presence of water under pressure: hexagonal plates, like mica, were also produced from the clay of Klingenberg, near Cologne. Fragments of wood were changed into lignite, and, at a higher temperature, into a black mass resembling pure anthracite. In general, says Daubrée, we distinguish the law, that, towards a red heat, the wet way develops in the production of silicates, analogous affinities to those manifested by the dry way; and that it makes other combinations which can in no way be formed by the dry way. He also observes, that there is hardly any rock which is impermeable to water, even if it does not already contain it. Senarmont produced, by the aid of water at a temperature from 130 to 300° cent., crystalline quartz, spathic iron, and most minerals which characterize veins (Daubrée, Études, pp. 52 and 90-4). The natural production of silicates in the wet way is nowhere better shown than at Plombières, in France. The bêton, which the Romans built around the hot springs at this place, was made of fragments of brick and sandstone (*grés bigarré*) without sand, and cemented with lime. This bêton was sometimes placed upon granite, and sometimes upon the alluvial soil; and, notwithstanding its extreme hardness, has been transformed by the prolonged action of mineral water, under no more

EVIDENCE CONCERNING THE NATURE OF TRAP AND BASALT.

The general teaching of geology is, that trap and basalt have solidified from a melted mass. The trap commonly occurs as a dyke, vertical, or nearly so, in all widths, from a mere line to many feet. It does not, like granite, send veins into the adjoining rock, but seems more like an upright tabular mass. Basalt generally occurs in more or less widely spread overlying sheets, often of great thickness, and frequently takes the columnar structure seen at Fingal's Cave and Giant's Causeway, the columns lying at right angles to the surface from which it is supposed to have cooled. These rocks have the same elements as granite, but in different proportions, having less

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than the ordinary atmospheric pressure, and at a temperature of no more than 60 or 70° cent. into what might be correctly called a metamorphic rock; and, though the water contains but a small quantity of foreign matter, it continually circulates, and thus a feeble action, long continued, has accumulated material enough for the change. By means of the alkalies, the water gradually re-acts on certain substances of the mass without any real dissolution, and produces silicates in the wet way, at a temperature at which they are reputed insoluble in water. The products depend on the nature of the mass in which the water re-acts: the same solution develops in each kind of rock special combinations, which shows that a part only of the elements were contained in the water, the complimentary ones existing in the mortar or the bricks. The minerals already in the rock meet the water half-way, to form the new mineral. Among the products, perhaps the most remarkable are the bubbles in the fragments of brick, lined with zeolitic crystals, making a perfect amygdaloid. There is a striking analogy, says Daubrée, between the crystalline silicates at Plombières, and those so often found in metamorphic rocks; and he concludes that it is to the wet, and not the dry way, that we must most often have recourse, to explain the origin of silicates in rocks: as such produces the various mixtures, crystallized independently of their relative fusing heats (Études, p. 107).

Mr. T. S. Hunt (Quart. Jour., 1859, vol. xv. pp. 489-496, "Chemical Geology") says, "The problem of the generation from the sands, clays, and earthy carbonates of sedimentary deposits, of the various siliceous minerals which make up the crystalline rocks, may now be regarded as solved; and we find the agent of the process in water holding in solution alkaline carbonates and silicates. acting upon the heated strata."

silica, but more soda, potash, lime, oxides of iron, and manganese; the texture is compact, and the color dark-green, brown, or black. Large quantities appearing in volcanic regions, its existence elsewhere has been attributed to a volcanic origin, though the ancient volcanoes may have disappeared. There is, however, some evidence opposed to this view. The experiments of Gregory Watt have often been quoted by geologists to prove the igneous origin of basalt, since he melted seven hundred pounds of that rock, which, upon cooling, took a columnar form. But Mr. Watt himself is by no means so decided upon this point as such persons would desire, or as they represent him. He says, "Though it is synthetically demonstrated that basalt may be formed by fire, the converse of that proposition stands supported by strong analogical arguments; and its formation by water must be allowed at least to be equally possible." He farther says in the same paper, "that perfect similarity of structure may exist in the products of aqueous and igneous formation," and that he does not consider the establishment of the peculiar modes of arrangement in basalt, as the slightest demonstration of the igneous origin of that rock.[1] Granite, it is well known, passes into greenstone and trap, and these last into clay-slate.[2] The bedded trap of Snowdon can hardly be regarded, geognostically, as any thing else than an ordinary stratified rock.[3] Mr. Murchison, in his Silurian System, says, "Besides the intrusive trap, and masses of altered rock, the Wrekin and Caer Caradoc throw off, at a little distance from their flanks, bedded sandstones and grits, which, although they sometimes contain organic remains, are in composition so analogous to the trap itself, that I designate them 'volcanic grits.'" And again he says, "Casts of encrinites, trilobites, and mollusca, are contained within a stone, which, if the fossils were excluded, would be said to resemble a sandy claystone of the trap family."[1] Professor Hitchcock describes a trap dyke in Vermont, which is thirteen feet wide, and contains in the central part of the mass, a conglomerate, made of pebbles from the size of a pea to four or five inches in diameter. These pebbles are composed of granite, gneiss, schist, quartz, gray sandstone, red sandrock, Trenton limestone, and Utica slate; and they are enough rounded to prove them to have been waterworn. The fragments of Utica slate are the most conspicuous, and contain enough carbonic acid to effervesce freely; which, Mr. Hitchcock remarks, shows that they could not have been exposed to so much as a red heat, say 1000° Fah., as this would have expelled the carbonic acid. He also observes, that an uprising melted mass, even if it tore off all of the various fragments above named,—which for a fluid to do is most unlikely,—would not bring them all together in one place towards the centre of the dyke, and make them rounded pebbles; and he concludes that they were introduced from above by means of waves and currents, which washed over the bottom of the ancient ocean. Finally, he remarks, that while veins of segregation and of injection do undoubtedly occur, all veins cannot be thus explained; and says, "We are satisfied that not a few of those granitic veins, which would be regarded by most geologists as veins of injection, had a different origin." And again, "If any should conclude from the views now expressed, that we should admit water, as more influencing the formation of almost every vein and dyke, even in volcanic regions, we should not object."[2] Mr. Hunt describes veins as wide in some cases as a hundred and fifty feet, which he believes to be veins of segregation; and, in other instances, he adopts Werner's idea, that fissures, filled from above by aqueous deposition, have produced what now appear to be intruded veins; and he describes one such a hundred yards long, and from four to seven feet wide, nearly vertical, composed of chert, occurring in a mass of syenite, in which, for a distance of two hundred yards from the vein, the felspar has been decomposed into a soft kaolin, thus producing a good example of a "contact change."[1]

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[1] Phil. Trans., 1804, vol. xciv. pp. 279–314, "Observations on Basalt."

[2] Lyell, Manual, p. 565, quotes Macculloch, Syst. of Geol., vol. i. p. 157.

[3] See Mr. Ramsay's section at the bottom of Murchison's map to Siluria; and Murchison's section on p. 92 of same work, ed. of 1859.

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[1] Sil. Syst., vol. i. p. 229-30.

[2] Geol. of Vermont, vol. i. p. 50; and vol. ii. p. 583-91.

The extremely narrow veins of trap offer the same objection to the idea of their having been intruded in a molten condition, that has been urged against those of granite; viz., that molten rock would not keep its fluidity long enough to fill very small fissures to great distances.[2] We have already recorded our dissent from the view, that veins have come to the surface from a central fluid reservoir; and if we deny the fluid injection of the very narrow veins, either of trap or granite, how can we avoid doing the same for the wider ones, of precisely the same chemical, mineral, and geognostical character? or where shall we draw the line where the igneous origin commences, and some other one leaves off?

Bischof concludes, as we have before seen, that the columnar structure of trap is plain evidence of a former heated state; but Professor Hitchcock describes beds of unconsolidated alluvial clay on the Connecticut, Westfield, and Deerfield rivers, which are divided by joints, "as perfect as any which appear in the older rocks," into doubly oblique prisms. These jointed beds are both overlaid and underlaid by strata of clay without joints, and, as Mr. Hitchcock observes, can never have been subjected to any pressure, except by the superincumbent alluvium; nor have they been disturbed in any way, nor subjected to heat. Thus shrinkage, high temperature, pressure, or fracture, cannot have made these joints. Mr. Hitchcock suggests "some such polar force as magnetism:"[1] whether such explanation be correct or not, there would seem to be no more reason for calling in a high degree of heat for the production of basaltic prisms, than for many well-defined concretions. Mr. Watt, in his paper before referred to, observes that, while contraction may explain the division into such rough prisms as are seen at certain places, it is utterly inadequate to illustrate the more perfect basaltic columns; that it offers no explanation for the extreme regularity of the sides, and the precision of the angles, — for the close contact and mutual adhesion of the columns, often so strong as to require considerable force to separate them, — facts, he says, in absolute contradiction to all idea of retreat or contraction.[2] We are not prepared to go so far as some have done, and refer the production of all phenomena to the action of water: at the same time we are strongly inclined to doubt the connection between dykes and any fluid interior of the globe. Geologists often make statements more in accordance with what they think *ought to* exist, than with what *does* exist; and they often make statements in accordance with nothing at all, because they think they must offer a solution for all phenomena.[3]

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[1] Canada Survey, pp. 35, 41.

[2] Bischof poured melted tin and zinc into narrow channels cut in sandstone slabs, and found that the wider the channel and the hotter the metal, the greater the distance to which the fluid penetrated. The greatest length of such a current, four lines wide and two and a half lines thick, was eight feet, which, Bischof observes, compared to a granite vein as thin as paper, shows that the latter cannot be of igneous origin; and to the remark that the granite veins may have been intruded into rocks already hot, which thus kept the injected matter fluid, he says that such could not have been the case in clay-slate, which shows these same mineral veins. — *Chem. Geol.*, vol. iii. p. 52.

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[1] Geol. of Vermont, p. 592.

[2] Paper on Basalt, in Phil. Trans.

[3] Thus, when Von Buch found basalt so related to the adjacent rocks as to preclude the belief that it was injected in a melted state, he asserts that it was protruded in a solid condition (Canary Isles, p. 412). Murchison speaks of the Malvern Hills as having been elevated in a solid state (Sil., p. 107). Macculloch gives an example of the protrusion of trap in a solid condition (Geol. Trans., 1814, vol. ii. p. 305, and pl. 13). Mr. Lyell, Manual, p. 583, quotes Sedgwick, Macculloch, and Cotta, to prove the protrusion of granite in a solid form; and remarks himself, that, if we admit this operation, we can explain many phenomena otherwise inexplicable.

CONTACT BETWEEN GRANITE AND OTHER ROCKS.

It has been generally stated with equal confidence and upon the same data regarding trap and granite, that both have been fluid ejections: indeed, Macculloch distinctly says, that, as geological agents, trap and granite are identical.[1] If, therefore, it should be shown that granite has not been a melted mass, we must conclude either that trap has not been ejected in a molten state, or that solid granite produces the same effect as melted trap. Let us now look at some of the phenomena of contact between granite and other rocks, in order to see if granite has been in a molten state. No region affords a better field for such investigation than that around Christiania in Norway, and no person has applied more time and talent to this important branch of study than Professor B. M. Keilhau.[2] He describes areas of granite and syenite of over two hundred square miles (English), lying neither over nor under sedimentary beds, but simply cutting them off, and standing as a great dyke, and sending veins into the adjoining masses. The granite, he says, cannot be traced to an eruptive fissure running in one direction, for it lies sometimes "transversely," and sometimes "longitudinally": the stratified rocks, adjoining, have an average dip of about fifteen degrees; but the granite has not at all disturbed the beds, which must have had the same position before the granite was present. Moreover, long tongue-shaped masses of the slate enter into the sinuosities of the granite, as do those of the granite into the slate. In some cases the slate at the contact is changed to siliceous slate, and the limestone to granular marble. But while in some locations the contact is clear and sharp, in other places there is a gradual transition from the clay-slate to granite. This important fact is minutely described by Keilhau, as it occurs at Sölvsberg, in Hadeland.

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[1] Syst. Geol., vol. ii. p. 100.

[2] Keilhau's papers are published in his "Gaea Norvegica," and in the first number of the "Nyt Magazin fur Naturvidenskaberne;" and a translation from the Danish may be found in the Edin. Phil. Jour., 1837-8, vols. xxiv. xxv., "Theory of Granite and other Massive Rocks;" and same work, 1844, vols. xxxvi. xxxvii., "Contact Products." The "Gaea Norvegica" may be seen at the Astor Library. The reader is particularly recommended to the original papers of this able and truly philosophical geologist.

"In the first place," observes Keilhau, "we have the usual siliceous slaty rock, into which clay-slate and calcareous clay-slate pass when near the granite boundary; a rock which shows distinct petrifactions, and in which stratification is particularly well marked. In another part we still detect the parallel structure; but, in the mass which in the preceding specimen seemed homogeneous, there now glimmer fine particles, that, in the following fragments, are to be recognized as plates of mica; and, in the succeeding ones, there also occur portions of felspar, and so forth. In short, there is to be observed a gradually advancing development from compact slate to a large-grained granular rock which Von Buch has expressly termed granite. But, however convincing our suit of specimens may be, it is so but in a small degree compared with the sight of the phenomena in nature itself: for, on the spot, we can distinctly mark that the transition is by no means sudden, but extends over a long distance; we can see plainly how gradually it takes place, and that the stratification and fossils vanish only by little and little, and all in proportion as the compact structure is converted into one which is more crystalline-granular; and I have no doubt that any one who will bestow time enough and attention will not only be able to follow the parallel lines of the slate into the incipient granite, but will also find traces of fossils where the siliceous masses present shining, microscopic, crystalline particles. "Now, the way in which certain hypotheses desire the separation between granite and sedimentary rocks to be understood, is in fact altogether contrary to nature. According to the common idea of the origin of granite (*i.e.* the igneous), there should be no gradual transition from it to unaltered rocks; and though the ramified, sharply defined boundary is the most common, yet the gradual passages *do* occur in thousands of cases."

There is another important petrographical relation in this Norwegian district. The granite masses are not fundamental rocks, but rest upon the ends of the primary schists,—the latter being penetrated by the former; and, what is strange, sometimes the junction between the granite and the schist is sharp and ramified, but at others shows a complete and gradual transition like that described above from clay-slate to granite. Now, as Professor Keilhau remarks, we cannot suppose a simultaneous origin of the primary and granitic rocks, for the latter rest on the upturned denuded edges of the former: nor can we conceive of the abrupt and the gradual passages as being simultaneous in any case.

The researches of Dr. Macculloch, at Glen Tilt in Scotland, have made us acquainted with one of the most remarkable exhibitions of the mutual relations of granite and sedimentary rocks that is anywhere to be found.[1] The phenomena at this place gave to Hutton some of his best arguments for the igneous origin of granite; but the researches of Macculloch have at least again opened the question. At the valley of the Tilt, alternating beds of limestone, quartz rock, and argillaceous schist come in contact with a mass of granite: the connection Macculloch describes as so complicated, that neither descriptions, drawings, nor specimens can give an idea of it. A fragment of the granite is often found entangled in the limestone, and a mass of the latter often intrudes into the former. The limestone is of a lead-blue color, large-grained, and highly crystalline. In some few cases, it preserves this aspect near to the granite; but more often, particularly when the beds have undergone elongation or contortion, the crystalline texture disappears, and it assumes the look of hornstone or compact felspar. But the most remarkable arrangement is the following: Parallel to the limestone bed or beds, and following every flexure and contortion which it undergoes with the utmost regularity, are alternating layers of a red siliceous rock, which Macculloch calls a modification of granite, and which he says must have been formed at the same time with the limestone; and he asks, "Must we then allow that there are cases where granite, or some matter resembling it, has been deposited like schist and bedded limestones?" and he holds it conceivable that a mass consisting of alternate layers of limestone and of micaceous and argillaceous schist might be acted upon by heat so as to change the siliceous layers into the red granite-like laminæ, while the limestone resisted any change. The whole paper of Dr. Macculloch is one of extreme interest, and of the greatest importance; both on account of the peculiar nature of the region which he describes, and because he was so well qualified to appreciate the phenomena.

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[1] Trans. Geol. Soc., 1816, vol. iii. p. 259–337, Pl. 13–22.

The extent of alteration, says Daubrée,[1] varies for the same rock and in the same country within wide limits, being especially well marked near granite: in some cases it has been traced as far as three thousand metres (about two miles). In the Pyrenees it reaches one thousand five hundred metres, with perfectly marked effects. Sometimes there occurs an entirely new molecular arrangement in the rock. Combustible elements are generally decomposed; lignite is changed to anthracite, and sometimes to graphite; more often there is developed new crystalline combinations either with the elements already in the rocks, or with new elements introduced. Sometimes the rocks bordering on granite are altered so as themselves to appear as an eruptive rock, and in some cases the intruding rock is itself altered by the contact. In other cases no change whatever is seen. Rose[2] describes a locality in the Altai, where granite overhangs and covers clay-slate; and, while the junction is everywhere sharp and distinct, no alteration is anywhere apparent at the contact: and Bischof observes, that, if this granite was originally melted, it must have been very liquid to have flowed over so wide an extent. Again, Keilhau describes a location in Norway, where fossils, with "perfectly distinct outlines," are found in marble next to granite.[1]

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[1] Études, p. 55.

[2] Reise nach dem Ural, referred to by Bischof (Chem. Geol., vol. iii. p. 53).

Such being the facts regarding the contact of granite with other rocks, we may next inquire whether granite has ever been a melted mass; whether it has ever been poured out from the interior of the globe, or injected as veins. This question will, we are aware, seem quite absurd to many persons who have taken the teaching of certain text-books as correct, without examination. The old doctrine that granite was the fundamental rock, and the notion adopted by Mr. Lyell, that any rock which gets depressed enough to be melted by the supposed central heat is ultimately changed to granite, have no foundation, if, as we believe, the lowest known rocks are the Azoic schists, which Mr. Dana says underlie every thing everywhere. There is no doubt whatever that granite overlies these old sediments in immense masses, and in masses so related to the adjacent formations that we can by no means suppose them to have been poured out from a molten interior. We have no proof that any granite has ever come up through these schists, or that there is any granite beneath them; but we have the most undoubted proof that granite is formed *in situ* by the metamorphosis of sedimentary rocks.[2]

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[1] Phil. Jour., vol. xxxvi. p. 356.

[2] Naumaun (Lehr., vol. ii. p. 174) says, "Die frühere Ansicht, dass der Granit das allerälteste Gestein der uns bekannten Erdkruste sei, und dass ihm daher vorzugsweise der Name eines Urgesteins gebühre, ist durch spätere fast in allen Gegenden der Erde ausgeführte Untersuchungen widerlegt worden," &c. Naumaun, however, believes in the plastic origin of granite, and asks what kind of plasticity it can have been? Studer (Edin. Phil. Jour., 1840, vol. xxix. p. 297) observes that the masses of granite which so abound in elementary treatises and upon colored sections should be considered merely as an article of belief, and not as an object of observation. The facts and conclusions of Keilhau, Naumann, Studer, Logan, Hitchcock, Dana, and others, are altogether opposed to the statement of Mr. Lyell, that granite rarely, if

There are two modes of looking at granite, the chemical and the geognostical; and the conclusions to which we come, as we use one or the other of these modes, may differ widely: it is only by a fair balancing of these conclusions, that we shall be able to obtain any thing like correct ideas on this subject. "The truths of geology and chemistry," says Keilhau, "can never be found in contradiction; but it may be, that we can, by the aid of geognostical facts, be able to form a decided opinion upon a subject which is still problematical in chemistry." He also observes, that, even if the chemist should produce a granite perfectly similar to that found in nature, the geologist is not bound to accept such mode of formation if it contradicts geognostical phenomena. "Chemistry must not account for facts by causes which contradict geognosy; but geognosy may contradict certain *applications* of chemical facts."[1]

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ever, rests *on* other rocks; and that the crystalline schists contain no pebbles, angular fragments, or organic remains. Mr. Jukes (Manual, p. 277) says, that no true granite will occur in regular beds interstratified with other rocks; but Mr. Hitchcock (Geol. of Vermont, vol. ii. p. 562) describes in Coventry, Vermont, ten several alternations of granite and clay-slate in slightly inclined beds.

[1] Phil. Jour., vol. xxv. p. 90. There exist "geological necessities," says Keilhau, which must be respected, though they should be opposed to some theorem of chemistry. In a reply to Berzelius, who concluded that granite could not be a metamorphosed clay-slate, because chemical analysis did not show the same component parts in both, Keilhau replies, that geological facts are so positive in some cases as to the transformation of the slate into granite, that absolute confidence cannot be given to chemical analyses, or rather to conclusions deduced from them, which do not agree with the geological transformation (Phil. Jour., xxviii. p. 378). Naumann also (Lehr., vol. i. p. 688) says, "Wenn also auch die Geognosie mit Dankbarkeit den herrlichen Tribut benutzen muss, welchen ihr die Chemie bereits dargeboten hat und noch fernerhin darbeiten wird, so kann sie doch niemals darauf verzichten, bei der Benutzung desselben den Maassstab und die Formen im Auge zu behalten, mit welchen ihr die Erscheinungen der Gebirgswelt entgegentreten. . . . Aber die Chemie allein kann nicht alle hierher gehörige Fragen vollständig beantworten, weil die Natur meist unter ganz anderen Verhältnissen und Bedingungen gearbeitet hat, als es der Chemiker in seinem Laboratorio vermag." — "Chemistry," says Studer, "is not yet in a condition to solve the higher problems in geology:

CHEMICAL AND GEOGNOSTICAL EVIDENCE REGARDING GRANITE.

The chemical evidence in favor of the igneous origin of granite is, that certain minerals crystallize from a melted condition: but we have already seen, from the experiments of Daubrée and Senarmont, that crystals of quartz and of other substances are also produced in the wet way at very low temperatures; so that, chemically, an aqueous origin is as probable as an igneous one. After the evidence furnished by Watt, Bunsen and Sorby, the order in which the minerals in granite have formed cannot be held as an objection to an igneous origin; though the presence of water, hydrated minerals, and minerals easily fused, is certainly opposed to such.[1]

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to deny facts, because chemistry cannot explain them, is to imitate those who rejected the laws of Kepler, until Newton had deduced them from gravitation" (Phil. Jour., 1840, vol. xxix. p. 299.)

[1] The mineral in granite most difficult to melt is the quartz, which, according to ordinary geological language, cooled or hardened the last; while the easily fused minerals have been formed the first, and have impressed their forms upon the more refractory. This has been explained by experiments, showing that silex, as it cools from fusion, does not pass at once from a fluid to a solid state, but remains in a viscous condition, while the temperature falls enough to allow the other minerals to harden and to stamp their shapes upon the quartz. Geological manuals generally refer to this order of hardening as something paradoxical; but Gregory Watt (Phil. Trans., 1804, vol. xciv. pp. 294-5) observes, "It by no means follows that the crystals afterwards found to be most infusible would be first generated. Their formation does not altogether depend on their greater or less fusibility, but on the relative strength of the attraction which unites them to the matter they are immersed in, and of the polarity which invites them to crystallize. In all crystallization from compound fluids, the order in which the several bodies crystallize must be determined by their relative quantities and attractions." Also on p. 300 he says, that "it is not only possible, but probable, that the most infusible substances might not be the first to crystallize." Mr. Sorby (Proc. Brit. Assoc., 1858, p. 108) says, "If it were supposed that the temperature at which minerals crystallized was the same as that of their own fusing points when heated alone, the interaction of quartz and tourmaline in granite would be a very unintelligible circumstance; but it can be easily explained

The geognostical evidence in favor of the igneous origin of granite is the occurrence of veins in such a manner as almost to preclude doubt of their molten condition and forcible injection, though we should remember that molten matter only occupies the rents made by some force acting previously to the injection. The geognostical evidence opposed to an igneous origin is that already adduced concerning basalt dykes; viz., that the smallest cannot have been injected; that the characters of the small ones and the wide ones are in many cases identical; and that it is very unlikely that any veins can have come from the supposed central fluid mass: also that granite lies in the vast masses above the schists, and in such petrographical relations to the adjacent formations as already described by Keilhau, and that it overlies unaltered rocks without producing the slightest change. Neither intruded veins nor contact changes can be considered as conclusive evidence of the igneous origin of granite. The conversion of uncrystalline limestone to marble, says Keilhau, is seen near masses that either never have had the high temperature, or at least not since they were in contact with the limestone. He also observes, that the phenomena of conversion appear in the greater number of cases in proximity to rocks whose former

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by supposing that the fused rock is simply a liquid melting at a high temperature, which is capable of dissolving various minerals, in the same manner as water dissolves salts." Professor Bunsen, of Heidelberg, considers those much in error who infer from the relative fusibility of the constituent minerals of granite, that it could not have resulted from igneous fusion; and says that no one seems to have taken into consideration, that the temperature, at which the substance solidifies when fused alone, is never that at which it solidifies when deposited from solution from another substance; and that the temperature, at which a chemically pure compound solidifies, depends not only on its own particular nature, but also and principally on its relation to that solvent. We introduce this note, not because we believe in the igneous origin of granite, but that the reader may not lack an important piece of evidence if he is inclined to hold such an origin. Professor Bunsen's paper appeared in the Zeitschrift der deutschen geologischen Gesellschaft, 1861, vol. xiii. p. 61-4, "Ueber die Bildung des Granites;" and a translation in the Quart. Jour., 1862, vol. xviii. part ii. pp. 11, 12.

melted condition is not undoubted, but is only for the moment assumed.[1] The excessively complicated junctions of granite and other rocks given by Dr. Macculloch in his "Western Isles," and his paper on Glen Tilt, are as hard to understand upon the assumption that the granite was in a fluid condition as that it was never so, and utterly incomprehensible upon either. In some cases, *sedimentary* rocks seem to send veins into the adjoining masses, which bear a striking resemblance to the so-called intruded veins of granite; and we might imagine that such would be the case. Suppose a bed of clay to be laid down, and by shrinking to be split, or to be fissured by disturbance: afterwards, let a sandstone bed be superimposed: the fissures would be filled with a new material: moreover, the surface of the lower bed might be somewhat decomposed even before the deposition of the new one: the contact may also be impregnated by certain elements of the new material incited to action by heat, moisture, and chemical operations, even before it becomes solid, and such alteration may affect also the new deposit, so that the contact modification may appear in one or both of the beds: finally, the whole may be metamorphosed, and we should have the appearance of a siliceous rock sending veins into an argillaceous one, and producing at the same time contact changes. Mr. Lyell gives an example of chalk strata at Norwich, England, which, from some cause, have become penetrated by deep cylindrical hollows, some of which are more than twelve feet in diameter, and sixty feet deep. The whole is now overlaid by sand and gravel, which has also filled the hollows; and he observes, that, while the central parts of the pits are sand and pebbles, the sides are lined with clay.[1] Now, if the whole were to be metamorphosed, the impression might easily be that the overlying rock had been ejected through the cylindrical opening, overflowing, and making a "contact change" at the clay-lining of the sides of the pits. In the peninsula of Melazzo, a new limestone reposes on granite and gneiss, which was full of fissures during the deposition of the newer masses: these rents are filled with the limestone, which at the contact is silicious, and firmly cemented to the granite. Beds of limestone near Dresden in Prussia have been laid down on syenite, in the fissures of which the limestone is converted into a fine granular mass, resembling hornstone. In Auvergne, tertiary beds rest upon granite, and are so mingled at the boundary, that the junction can with difficulty be distinguished.[2] Limestone, says Cotta, has often been so softened as to be squeezed into the fissures of neighboring rocks, so as to form veins and ramifications, and also to enclose fragments of the invaded rock: this softened state, he observes, was succeeded by a granular-crystalline consistency, accompanied by the formation of substances at the junction of the masses which chemistry may readily explain, called contact products.[3] The Laurentian limestones, Mr. Hunt says, sometimes embrace broken and contorted fragments of quartzite, and penetrate like a veritable trap into the fissures of gneiss.[4] Dr. Macculloch remarks, that, since changes take place in sedimentary rocks near to granite, it has been supposed that the granite produced such changes; but that there are places where schist and limestone bear a great resemblance to such veins, as, in the case of granite, may be traced from a mass of that substance into the neighboring rocks; yet that there is no doubt that both the limestone and the schist are stratified rocks: and he observes that he has seen the same appearance in clay-slate.[1]

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[1] Phil. Jour., vol. xxxvi. p. 355, "Contact Products."

The passage of granite into the stratified rocks at the boundary, and the ramifications we observe at other points, remind us very distinctly, says Keilhau, that all these phenomena, even to the minutest detail, are only a repetition of the appearances presented by dolomite in its relation to the surrounding stratified rocks; and he thinks that the same theory that is applied to dolomite ought also to be applied to granite (Phil. Jour., 1838, vol. xxv. p. 95).

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[1] See fig. on p. 82, Manual, Am. ed., 1861.

[2] Keilhau's Contact Products, Phil. Jour., 1844, vol. xxxvii. p. 144.

[3] Cotta, in Zeit. der deut. geol. Ges., 1852; translated in Quart. Jour., 1853, vol. ix. part ii. p. 15.

[4] Canadian Naturalist, 1858, vol. iii. p. 194, "Igneous Rocks and Volcanoes."

CONCLUSION REGARDING THE CAUSE OF CONTACT CHANGES.

The evidence furnished by the contact between granite and other rocks seems even more opposed to the idea that the presence of a molten mass has been necessary for the metamorphism, than does that regarding trap. If, therefore, Macculloch is correct in making trap and granite identical as active geological agents, the above evidence is opposed to the igneous origin of trap. Dr. Mantell remarks, that, when granite has been erupted in a fluid state, it has invariably altered the adjacent rocks; but that, when consolidated masses have been protruded, no such changes are seen.[2] But the granite which Keilhau says cannot be traced to an eruptive fissure, and which overlies the fundamental schists, shows the most extreme contact change; while the large, overlying mass, above referred to by Bischof, and which he remarks must have been very fluid to have flowed so far, if it was poured out in a molten condition, has produced no change at all at the contact with the slate below. So far, therefore, as the explanation of metamorphism depends upon the supposition, that the contact changes at trap and granite junctions have been caused by the presence of highly heated or melted masses, the evidence appears to fail altogether. It seems indeed incredible, that any mass of even molten rock could produce the mineral changes which are seen at a distance of one or two miles from the contact, while a large stream of red-hot lava flowed over a bed of ice at Mount Etna without melting it. Mr. Lyell explains this fact by supposing that a layer of volcanic sand and scoriæ protected the ice from the action of the lava;[3] but we should suppose that one hundred feet of rock would be at least as bad a conductor of heat as a thin bed of ashes. Mr. Nasmyth showed that a large plate-iron pot, lined with a coating of clay and sand half an inch thick, stood for over twenty minutes with eleven tons of white-hot melted cast-iron, without becoming inconveniently hot to the hand on the outside.[1] This fact, Mr. Lyell says, may explain how strata, in contact with dykes or beds of fused matter, have sometimes escaped alteration; but Mr. Nasmyth remarks, that the radiation from the outside carried off the heat as fast as the slow-conducting power of the sand and clay transmitted it. Of course, in the case of a dyke of fused matter, the heat would not be radiated, but would be conducted at once into the rock which is to be altered. As to the transmission being slow, that would seem to be a matter of no importance, since we are in no way limited as to time. If contact changes are in any way due to heat, it would seem that such heat must come in some way different from that generally supposed; for the changes occur where no heated mass has been present, and, in many cases, commence at a distance too great to suppose that the contact caused it. Mr. Sorby remarks, that when a dyke or vein of intruded matter changes the rocks through which it passes, near to the contact, but not at a distance, there can be no difficulty in assigning the effect to its cause, though there may be a doubt as to the manner in which the cause has acted.[2]

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[1] Geol. Desc. of Glen Tilt, Trans. Geol. Soc., 1816, vol. iii. pp. 259–337.

[2] Wonders of Geology, 1848, pp. 859–60, 6th English ed.

[3] Lyell's Principles, ch. xxv. p. 413, Am. ed.

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[1] Quart. Jour., 1847, vol. iii. p. 232.

[2] A mass of rock under immense pressure might be subject to a heat which under ordinary circumstances would melt it. Now, if such heat had an origin separate from the pressure, we might suppose, that, in case of a fracture, the pressure would be suddenly removed, and that the rock adjacent to the fracture would melt; but if we suppose the heat to be developed *by the pressure*, then, if the pressure was removed, the heat would be removed also. Still the pressure might be taken away suddenly; and, if the equilibrium of temperature was unable at once to restore itself, there might be some alteration from heat. M. Keilhau (Phil. Jour., vol. xxxvii. p. 158) says, "When two heterogenous masses touch each other, forces are brought into operation by means of which chemical products are called forth;" and asks, "But is it not also probable, that, in the formation of a fissure, the tearing asunder *of one and the same rock* disturbs the tranquillity of these forces, and brings

KEILHAU'S THEORY OF GRANITE.

Many good geologists, believing that granite in its structure, and more especially in its relations to the adjacent rocks, shows no evidence of ever having been in a melted condition, or of having been produced at any great depths, have supposed that it was no more than a sedimentary rock altered *in situ;* or an extreme product of metamorphism, in which the original structure has been entirely changed for a new one. Professor Keilhau has proposed the following hypothesis to account for it. After the primitive schists (Laurentians) had been formed, and thrown into the highly inclined positions they now occupy, clay-slates, limestones, and sandstones were deposited thereon. These last were afterwards disturbed, producing the high angle we find at present. Then, large parts of these newer-stratified tracts began, by "tranquil processes," to become converted into massive mountain rocks. The clay-slates became granites and syenite, and the sandstones became porphyry. Confirming this, it is stated, that there exists a fixed place in certain stratified deposits of a particular kind for the massive rocks. "It is not a mere conjecture, but a fact," says Keilhau, "that the presence of the sandstone formation affords the conditions required for the occurrence of the great porphyry masses," and "that a similar dependence exists between the granitic rocks and clay-slate."[1] The

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them into action?" Of course, a relief of pressure would cause an alteration in the internal conditions of the rock, and such alteration would be felt to a greater or less distance according to the ability of the rock to transmit a force; and this would depend upon its rigidity.

[1] Mr. Dana, speaking of the new red sandstone from Nova Scotia to South Carolina, says that the trap is a singularly constant attendant on the sandstone, and everywhere bears evidence of having been thrown out soon after the deposit of the sandstone, or in connection with its later beds (Am. Jour., 1856, vol. xxii. p. 322 *et seq.*); and, in his manual he observes, that it is remarkable that the trap should occur just where the triassic beds exist, and only sparingly east or west (p. 430). Mr. Hall also observes, that, in the Lake Superior region, extensive trap formations are found in the period of the Potsdam Sandstone; in

petrographical characters of the rocks are modified in a determinate manner, according as the masses lie near to, or higher above, the base of the series. Towards the bottom, we have almost pure quartz, in which felspathic substances are very rare; but, higher up, the basis of the rock acquires more felspathic matter, and becomes first euritic, and afterwards quite crystalline. The regularity of these changes is limited only by another remarkable rule, that the larger masses are more crystalline and richer in felspar than the smaller ones; so that a perfect porphyry may be seen near the primary rock, provided the mass be large enough, while a quartzose and rather euritic rock may occur farther on in the range of beds, because it exists in a comparatively thin mass.[1] Solid, unorganized bodies are capable of changing their chemical and structural characters, by means of slowly working causes in the solid rock, and of assuming forms which they did not before possess; in fine, of appearing as entirely different bodies, without having been in a fluid state.[2] This idea of long-continued, gradual operations within the rock Keilhau regards as the

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Nova Scotia, with the new red; and in the Connecticut and Hudson valleys connected with a newer sandstone. He also describes beds of stratified amygdaloidal trap alternating with shale and sandstones (Introduction to Pal., p. 78-80).

[1] Edin. Phil. Jour., 1838, vol. xxv. p. 263.

[2] This fact is well shown by a metallurgical process, which has been long employed, and is described by Brieslak. Poor copper ores are brought to a heat *which must not be high enough to melt them*, when the copper, which before existed as copper pyrites, and was more or less uniformly disseminated through the mass, is found as a kernel in the interior; while portions of the iron and sulphur, contained in the crude ore, are driven outwards. This, which is only one of many facts of the same kind, however it may be explained, shows that chemical and morphological changes may take place in what we call rigid bodies; so that we are not bound to believe that granite must have been in a fluid state, wet or dry (Phil. Jour., 1838, xxv. p. 93). Iron often undergoes an entire change: from being a tough and fibrous bar, it becomes, from long-continued impact, a brittle, granular mass; a condition often seen in broken railway axles. See also Hood, on "Changes in the Internal Structure of Iron." Phil. Jour., vol. xxxiii. p. 286; and Warrington on "The Re-arrangement of the Molecules of a Body after Solidification."

"foundation of the whole of geology;" and he applies it not only to granite in mass, but also to large veins. He does not suppose that the materials necessary for the new minerals were obtained from any point *without* the mass in which such change takes place, nor yet that they originated at the point where the new substance is found, but that they were *conveyed* thereto; and that we must therefore admit certain attractions as the active moving agent which prepared the requisite space for the new matter. The metamorphosis of limestone into crystalline marble, he observes, occurs in such relations as to preclude the idea that the change has been other than a gradual operation performed in the solid rock.

MR. HUNT ON THE LAURENTIAN ROCKS.

M. Keilhau's theory, which is substantially the same as that proposed in 1834 by Christian Keferstein,[1] is adopted by Mr. T. S. Hunt, whose papers in various publications are among

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Same Jour., p. 292. Gay Lussac, in his "Cours de Chémie," observes, "Dans un corps solide les molécules peuvent changer de position, et prendre la forme crystalline." Gregory Watt, in his paper before referred to, says, that solution, far from being necessary to crystallization, effectually prevents its commencement; for, while solution subsists, no crystallization takes place: and he goes yet farther, and thinks it doubtful whether *previous solution* be essential. His experiments on basalt show plainly, that very marked changes of internal arrangement take place in what we call solid bodies. In the seven hundred pounds of basalt which he melted, the first result was a liquid, but tenacious glass. The mass was very slowly cooled, when minute globules formed, which, after a while, gave way to a perfectly homogeneous and compact texture: following this state commenced the development of a second set of spheroids, some of them two inches in diameter, with a radiate structure; these, as they enlarged, came in contact, and compressed each other into irregular polyhedrons. This texture again gave place to a granular one: brilliant points appeared, which became more and more numerous, finally arranging themselves into regular forms, so that the whole mass became pervaded by thin, crystalline laminæ. At the same time, the magnetic influence of the rock increased, till it became so powerful that fragments were suspended by the magnet.

[1] Naturgeschichte des Erdkörpers in ihren ersten Gründzugen. Leipzig, 1834. 2 vols. 8vo.

the best chemico-geological essays that have anywhere appeared, and to which the reader is referred for many interesting and important considerations.[1] The Laurentian rocks of Canada consist of vast beds of limestone, schists, gneisses, and of what would be generally called granite, — a coarse-grained aggregate of quartz, felspar, mica, and other minerals, but showing in the field obscure traces of bedding. This granitic rock is found in masses several thousand feet thick, rising into high peaks and ridges, and would be mistaken, says Mr. Hunt, for an intrusive igneous rock, while it is, without doubt, an altered sedimentary one. Masses of quartz, from four hundred to six hundred feet thick, are also referred to the same origin.[2] There is, according to Mr. Hunt, no evidence of any granite substratum to the Laurentian system: the granites are simply sediments which have been metamorphosed. When they lie in their original position, he calls them indigenous; but, when so altered as to be forced in among other rocks, exotic.[3] The metamorphosis, Mr. Hunt says, has been made in two modes, — first, by molecular changes, that is, by crystallization and re-arrangement of particles; and, second, by chemical re-action between the elements of the sediments. By these means, silicates are generated from sands and clays, as so plainly shown in contact products. The sediments under heat, water, and pressure, are made more or less plastic, according to the intensity of the forces, even enough so as to be forced as intrusive veins into broken strata; and not only rocks of that mineral composition which have been called igneous are thus found, but also limestones.[1] The only distinction, says Mr. Hunt, between the stratified and the unstratified masses, must, in most cases, be based upon their position, and relation to the adjacent rocks. Indeed, he says that different portions of the same mass may seem exotic or indigenous, according as it has been more or less acted upon.[2] Mr. Hunt also believes that veins may be aqueous deposits filling the fissures, which they occupy, and there consolidated and crystallized. The idea that all of the so-called igneous rocks originate in sedimentary beds he considers as accounting for the various products of volcanoes, which vary so much in different places at different times; as also the occurrence of infusoria in the ejected rocks.[3] The metamorphism of sediments *in situ*, their displacement in a pasty condition from igneo-aqueous fusion as plutonic rocks, and their ejection as lavas, with attendant gases and vapors, are considered by Mr. Hunt to depend upon the difference in chemical composition of the sediments, and their temperature and depth.[4]

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[1] Igneous Rocks and Volcanoes; Canadian Naturalist, 1858, vol. iii. p. 194.

Some Points in Chemical Geology; Quart. Jour. Geol. Soc., 1859, vol. xv. p. 488.

Chemical and Mineral Relations of the Metamorphic Rocks; Am. Jour., 1863, vol. xxxvi. p. 214.

Metamorphic Rocks, ch. xvii.–xx., Canada Survey, and papers on "Lithology," Am. Jour., 1864, vols. xxxvii. and xxxviii.

[2] Canada Survey, ch. iii. and xix.

[3] Am. Jour., vol. xxxvi. p. 226.

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[1] Can. Nat., vol. iii. p. 196.

[2] Am. Jour., vol. xxxvii. pp. 252 and 255.

[3] Can. Nat., vol. iii. p. 199.

[4] Quart. Jour., vol. xv. p. 496.

Mr. Scrope (Quart. Jour., 1856, vol. xii. pp. 326–50) supposed that the water which is known to be present in all rocks, might, from great pressure, remain unvaporized, and impart a viscidity to the granite; in which pasty condition the mass might swell, fracturing the overlying rocks, and, becoming forced into the rents thus made, appear as intrusive veins. Scheerer (Bull. Soc. Géol., vol. iv. p. 468) attributed the plasticity of granite to the combined action of heat and water; which latter he considered as "primitive," and not infiltrated subsequently to the production of the rock. Professor Houghton (Proc. Brit. Assoc., 1863, pp. 48–70) says, that in the centre of a granite area thirty miles long, in Donegal, the rock in hand specimens is a true granite, but in the field is seen to be stratified; the strike of the beds agreeing with that of the uncontorted sedimentary beds of the country. In some cases, portions of highly contorted gneiss are found actually within the granite. But this same Donegal granite is, in many places, apparently intrusive, and sends out veins. Mr. Blake (vol. v. part ii. of Pacific Railroad Explorations, p. 199), speaking of the Tejon pass of the Sierra Nevada, says, "The rocks generally classed as metamorphic, gneiss, mica slate, &c.,

SORBY'S MICROSCOPIC EXAMINATION OF GRANITE.

Several geologists have endeavored to show, by the microscopic examination of rocks, the condition under which they were formed. Davy, Brewster, Sivright, Nichol, and, later, Sorby and Bryson, have devoted themselves to this study. Ten years of experiment, Mr. Bryson says, upon the structure of rocks relative to their formation (more particularly granite), shows that igneous rocks have a radiated stellate form; while granite never has this, but, instead, has cavities containing fluid. These cavities occur in quartz crystals, quartz veins, in the quartz of granite and schist; and also in topaz, cairngorm, beryl, tourmaline, felspar, and other minerals. They are seldom entirely filled with the fluid, because, since the formation of the cavity, the fluid has contracted; and, by again heating the rock till the cavity is filled, we find the temperature at which it was originally formed. Mr. Bryson says that many hundreds of experiments show that at 94° Fah. the fluid was expanded enough to fill the cavity; whence he infers that such was the temperature of formation.[1] Mr. Sorby commences by saying, that crystalline substances possess sufficiently characteristic structures to show whether they came from solution or from fusion.[2] The means by which he judges are the

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are the predominating forms. These rocks pass by insensible gradations from one to the other, and into compact granite and syenite; and even these last rocks present traces of lamination or structure which connects them with the others." Delesse (Quart. Jour., 1860, vol. xvi. part ii. p. 12) says, "It seems to me that granite presents none of the characteristics of igneous rocks. For the development of its minerals, it was sufficient that it formed a magma slightly plastic; the study of the granite in some places showing that it could crystallize in nearly a solid state." Mr. Hall says (Introduction, p. 75) that the masses of what is called eruptive or intrusive granite in the Appalachians are almost certainly, in all cases, a modification of some pre-existing sedimentary rock, and furnish no evidence of having been derived from a primary mass, or erupted in a state of fusion.

[1] Edin. Phil. Jour. 1862, vol. xv. pp. 52–63.

[2] Quart. Jour. Geol. Soc., 1858, vol. xiv. pp. 453–500.

cavities above referred to. These are exceedingly minute; in some cases being only one-thousandth of an inch apart, or a thousand million per cubic inch. From the proportion between the cavity and the vacuity, and assuming that granite finally consolidated at about 600° Fah., Mr. Sorby concludes that the various granites were formed under a pressure equivalent to a depth of from forty thousand to sixty-nine thousand feet. He also says, that the mean relative size of the vacuities in the quartz of the slightly metamorphosed schists of Cornwall, at a considerable distance from the granite, is different from that as the granite is approached, and indicates a heat, increasing from only about 392° Fah. at a distance, to a heat nearly as high as that at which the granite consolidates; a fact, says Mr. Sorby, which agrees with the gradual passage from gneiss to granite, and might be used as a strong argument by those who contend that some granites are only thoroughly altered stratified rocks. As the metamorphic rocks near to granite are thus supposed to have crystallized at about the same temperature as the granite, Mr. Sorby considers it likely that the temperature concerned in the normal metamorphosis of the gneissoid rocks was due to their having been at a sufficient depth beneath the superincumbent strata. The granitic dykes in Cornwall, termed "Elvans,"[1] according to the above process, were consolidated under a pressure equivalent to from forty thousand to sixty-nine thousand feet of rock. These aqueous solutions, Mr. Sorby thinks, were caught up during the formation of the crystal, and have remained ever since hermetically sealed up in the solid substance, without increase or diminution of the fluid; an opinion confirmed by the fact that specimens taken from below low water in the sea, and afterwards kept in water, are no different from those of very thin fragments, kept dry for years. The water is thus, as Scheerer expresses it, "primitive." "On the whole," says

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[1] Elvans are veins of quartziferous porphyry, differing from granite chiefly in the lack of mica. They are common in Cornwall.

Mr. Sorby, "the examination of the constituent minerals of granite indicates a formation, combining at once igneous fusion, aqueous solution, and gaseous sublimation. The proof of the operation of water is quite as strong as that of heat; and, in fact, I must admit that, in the case of coarse-grained, highly quartzose granites, there is so little evidence of igneous fusion, and such overwhelming proof of the action of water, that it is impossible to draw a line between them and those veins, where in all probability mica, felspar, and quartz have been deposited from solution in water, without there being any definite igneous fusion, like that of furnace slags or erupted lavas. There is thus in the microscopic structure a most complete and gradual passage from erupted lavas, indicating a perfect and complete fusion, to simple quartz veins, having a structure precisely analogous to that of crystals deposited from water; while between these extremes are all the connecting links, the central link being granite: and my own observations in the field cause me to agree with Elie Beaumont, in concluding that there is the same gradual passage on the large scale."

Mr. Sorby evidently takes it for granted, that granite was consolidated from a soft state; and the relation between the cavities in the granite and the schist, above noticed, would certainly seem to show that both were produced at the same time. Now, while a schist, which is only an altered sediment, may have been, and probably was, soft when it was formed, we can hardly suppose that it was so when metamorphosed. Yet the cavities seem to be connected with the metamorphism, since they change in character as the granite is approached. If we assume the heat in these cases, we can deduce the different pressures to which the rocks have been subjected; or, if we assume the pressure, we may deduce the heats. If the temperature of consolidation were higher, of course the pressure would be greater. If the elvans above mentioned were produced under an enormous pressure, we should suppose that such would also have affected the sedimentary rock into which the elvan penetrates; but such is not the case, the latter being unaltered, except a slight "contact change."

PROFESSOR EDWARD HITCHCOCK ON THE CRYSTALLINE ROCKS.

Professor Edward Hitchcock, sen., in the latter part of his life, published some remarkable facts, and drew therefrom some very important conclusions, concerning the crystalline rocks, which are, at the same time, an extremely valuable contribution to science, and a splendid example of a noble devotion to truth. This distinguished man, it is well known, was foremost among what we may call Plutonists; and, through a long and active geological life, taught the igneous origin of granites, the intrusion of molten rock, and metamorphosis by heat; yet, upon the discovery of new facts, he did not hesitate to give up these old ideas, and to adopt entirely new ones, in accordance with the more recent developments. The facts and the conclusions to which we refer, may be found both in the Report upon the Geology of Vermont, and in the American Journal of Science.[1]

"The granitic rocks," says Mr. Hitchcock, "have almost universally been regarded as intrusive rocks; that is, as thrust, in a melted state, into cracks and between the strata of other rocks. But the opinion is now gaining ground, that, in many cases, perhaps in nearly all, they are merely stratified rocks, which by heat, or the joint action of heat and water, have lost their stratification, and assumed new crystalline forms. They are, in fact, an extreme product of metamorphism." Mr. Hitchcock also observes, that if we admit the Azoic schists to have been brought into a plastic state since consolidation by the agency of water, we may extend the same conclusion to granite, as they pass by insensible degrees from one to the other; and he says, "We cannot, upon the whole, but regard these new views of the origin of the granitic rocks as a decided advance upon those long held, and as giving a far more satisfactory explanation of the phenomena. We understand the theory to admit, and not to deny, the igneous fluidity of the central parts of the earth." He farther observes, that "whenever we find masses made slaty, schistose, and foliated by metamorphic action, we may look for the granitic rocks; and the more thorough the metamorphism, that is, the more crystalline the strata, the more likely are we to meet with granite. We cannot resist the conviction, from what we have seen, that the most complete metamorphism produces fine granite; while a less perfect fusion results in other varieties of the granitic group. The conclusion seems to follow closely from these facts, that the production of the granitic rocks is intimately connected with the metamorphosis of the stratified ones; we do not say as cause and effect, but as joint products of the same general causes.[1] We believe the internal heat of the earth to have been combined with water in both cases. But, in the case of the granitic rocks, the heat was enough for entire fusion; while, in the stratified, it did not destroy the marks of original structure."

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[1] Vermont, pp. 28–50 and 572–6; and Am. Jour., vol. xxxi. p. 372, "On the Conversion of certain Conglomerates into Talcose and Micaceous Schists and Gneiss, by the Elongation, Flattening, and Metamorphosis of the Pebbles."

Professor Hitchcock describes conglomerates which show all stages of alteration, from a simple flattening of the pebbles, to a more excessive action, which has converted them into the silicious laminæ of the schist and gneiss, and the cement of the mass into mica, talc, and felspar. This process he regards as both chemical and mechanical; out of the cement of the conglomerate, chemical action has produced mica and talc, the parallel structure of which, he thinks, resulted from pressure. "Looking at the edge," he says, "we see alternate laminæ of quartz and talc, or mica, and pronounce it a good example of talc schist; but a fracture at right angles shows the flattened pebbles. Let the process be carried a little farther, and we have no evidence that they ever were pebbles." Mr. Hitchcock also describes conglomerate with a cement of syenite or granite, or as a syenite or granite containing pebbles; and he also gives an example where conglomerate passes into porphyry, and thence into granite, all forming one undivided ledge: "so that the conclusion is forced upon us, that the granite and porphyry have been formed out of the conglomerate."

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[1] Delesse observes (Quart. Jour., 1861, vol. xvii. part ii. p. 18), that the plutonic rocks are the effect, and not the cause of the general metamorphism; that if metamorphism be very energetic, it gives rise at once to both metamorphic and plutonic rocks: which latter have often behaved in an eruptive manner, filling fissures in other rocks.

CONCLUSION REGARDING THE CAUSE OF METAMORPHISM.

The hypothesis of metamorphism by heat depends upon the analogy, which certainly appears to exist, between the so-called metamorphic rocks and contact products. When, however, we look at the chemical and geognostical facts, we find it impossible to conclude that granite has been a melted mass, or has ever had the supposed high temperature; and, as the effects of granite and trap have been considered similar, as indeed they seem to be, we cannot suppose, that, in the case of trap, the change has been produced simply by contact with a heated mass. Some trap dykes have most likely been formed by aqueous deposition from above; and we cannot attribute with certainty an igneous origin to any. Indeed, some masses of granite and trap have produced no change whatever at the junction with the rocks they invade, while marked changes *do occur* at the contact with rocks, which we know have never been melted, and which we have no reason to suppose were ever hot. We may therefore conclude, that simple heat has not been the agent of metamorphism; yet the effects produced are, in many cases, such as we might suppose would result from a high temperature. We must therefore look for some more general cause, of which possibly heat may be a result, standing as an intermediate between such general cause and the final effect, and sometimes acting and sometimes not;

and it appears to us, that such general cause may be found in the enormous pressure which must always be present in slowly sinking masses of sediment.

In the preceding, we have approached the subject of metamorphism, as is generally done, upon the most difficult, that is, the most metamorphic side. Let us now approach it on the side of the least change; for as the metamorphism becomes more extreme, so do the producing causes become harder of recognition.

CLEAVAGE AND FOLIATION.

There is a class of phenomena which occur in nature, and which may also be produced by art; and which may, perhaps, be received as the first and most simple form of metamorphism. We refer to the superinduced structure called slaty cleavage,[1] — a structure by which the mass of certain rocks, apparently homogeneous, may be split into thin sheets, as shown by the ordinary roofing and writing-slates; such divisions being in no way dependent upon the original beds of deposition, but most often lying at a high angle to them. This structure must be distinguished from both foliation and lamination. By foliation is denoted that separation of the different minerals into distinct layers, which so marks the schists and gneisses. Such layers may or may not be separable. Lamination is the division into thin plates by the separation of the original layers of deposition. The last is simply the result of the original formation of the rock; but cleavage and foliation are independent of the operations which formed the rocks, and were impressed upon them subsequently, and thus belong to metamorphism. Cleavage is best seen in clay-slates, and like fine-grained rocks. When beds of sandstone, limestone, &c., alternate with slaty strata, the cleavage is mostly confined to the latter, though it often divides the former into roughly tabular masses. Where cleavage cuts through conglomerates, it does not cut through the pebbles as joints so often do, but leaves them standing out in relief. While cleavage planes are independent of stratification, they preserve a parallelism among themselves over very great distances. The strike of the cleavage planes, generally viewed, coincides with the main lines or axes of disturbance; though locally it may vary: and it is important to observe, that it is a phenomenon intimately connected with tracts of folded and plicated rocks, otherwise with mountain regions.

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[1] The principal papers upon cleavage are as follows: Sedgwick, Trans. Geol. Soc. 1835, vol. iii. 2d ser. pp. 461–86. Darwin's South America, vol. iii. of the Geology of the Voyage of the Beagle, ch. vi. pp. 140–68. D. Sharpe, Quart. Jour., 1847, vol. iii. pp. 74–105; vol. v. pp. 111–29. Sorby, Edin. Phil. Jour., 1853, vol. lv. pp. 137–48; and Phil. Mag., 1856, vol. ii. pp. 20–36. Tyndall's Lecture before Roy. Inst., 1856; Phil. Mag., vol. xii. pp. 35–48. Harkness and Blyth, Phil. Jour., 1855, vol. ii. pp. 245–62. Phillips, in Report of 26th Meeting Brit. Assoc., 1856, pp. 369–96. Rogers in Edin. Phil. Trans., 1857, vol. xxi. p. 458. Portlock's Address to Lond. Geol. Soc. for 1857. Sorby, in Proc. Brit. Assoc., 1857, p. 92. Murchison and Geike, in Quart. Jour., 1861, vol. xvii. p. 232. Daubrée, Études sur le Métamorphisme, pp. 65–6 and 111–15.

Many hypotheses have been proposed to account for the phenomena of slaty cleavage. Professor Sedgwick supposed that crystalline or polar forces had acted upon the whole mass of cleaved rock simultaneously in given directions; and that the planes were the result of the ultimate chemical arrangement of the particles of the rock, and, in most cases, unconnected with any direct mechanical action. And he observes that we need not wonder at the extent to which such changes have been carried, when we bear in mind that the sediments may have been for thousands of years under a pressure of a thousand atmospheres. This crystalline nature of cleavage he considers confirmed by the fact, that the planes are coated over with flakes of chlorite and semi-crystalline matter, which not only define these planes, "but strike in parallel flakes through the whole mass of the rock."

Mr. Darwin, who had ample opportunity in South America to examine the phenomena of cleavage on a very large scale, gives many valuable facts regarding a connection between cleavage and foliation. In several places he saw the fine laminæ[1] of clay-slate cutting straight through the beds, and thus unmistakably cleavage, differing slightly in their color and in compactness, some of the laminæ being more jaspery than others; and he concludes, that the same cause which has produced the fissile structure has altered the mineral character of the rock in the same planes. From cleavage he passes to foliation, and remarks, that, like cleavage, it preserves a uniform strike, parallel to the axis of elevation, over very large areas; that, when masses of fissile and foliated rocks adjoin, the cleavage and foliation are generally parallel. The idea that the folia of quartz, mica, &c., in the foliated schists, were separately deposited as different sediments, and then metamorphosed, Mr. Darwin considers untenable; as he found such folia in one mass, parallel to cleavage in an adjoining mass, the cleavage being perpendicular to bedding. Besides, Mr. Darwin gives an example to show that the folia of different minerals can originate through the metamorphosis of a homogeneous fissile rock, and *that* without any melting of the mass. Mr. Darwin concludes as follows: "Seeing, then, that foliated schists indisputably are sometimes produced by the metamorphosis of homogeneous fissile rocks, and that some fissile and almost homogeneous rocks show incipient mineralogical changes along the planes of their cleavage, and that other rocks, with a fissile structure, alternate with, and pass into varieties with a foliated structure, — I cannot doubt, that, in most cases, foliation and cleavage are parts of the same process; in cleavage there being only an incipient separation of the constituent minerals, in foliation a much more complete separation and crystallization." And again: "The planes of cleavage and foliation are intimately connected with the planes of different tension, to which the area was long subjected, after the main axes of disturbance had been formed, but before the final consolidation of the mass, and the total cessation of all molecular movement."

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[1] Mr. Darwin uses the word *laminæ* to denote the sheets of slate produced by cleavage.

Mr. Daniel Sharpe devoted much time to the study of cleavage; and while his geognostical conclusions have been, we think, justly condemned by leading geologists, his elucidation of the details of cleavage structure, and his conclusion as to the main cause of the phenomenon, are, we think, entirely correct. From the distortion of fossils in cleaved rocks, which are flattened out in a direction parallel to the cleavage planes, he infers that the cleavage has been produced by intense compression, acting perpendicularly to the cleavage planes; and he agrees with other authorities that slates are most affected, limestones but slightly, and soft earthy beds not at all. He explains the fact that the cleavage planes sometimes coincide with, and at others differ from, the direction of the bedding or stratification, by supposing that two separate operations have occurred at different times, of which the last was the most general, and produced cleavage. The mechanical structure of the unfossiliferous cleaved rocks, he says also, shows that they have been compressed; and, where the cleavage planes are interrupted by beds of quartz obliquely inclined to them, they do not go through the quartz, but are jammed and bent against such. Mr. Sharpe argues strongly against the idea that any crystallization has produced cleavage; and remarks, that crystals of pyrites, and other rhomboidal crystals of various minerals, occur in cleaved slates, without their sides being based upon the cleavage planes, or having any definite position in the rock. He observes also, that, in passing from true slates through the metamorphic rocks to the thoroughly crystalline ones, we always find the cleavage more developed as the rock is less crystalline. In the best roofing slates, where the cleavage is strongest, there is no crystallization; in the metamorphic slates, the cleavage is fainter as the crystals are more abundant; and, on reaching a thoroughly crystalline rock, the cleavage disappears entirely. This alone, says Mr. Sharpe, is sufficient proof that cleavage had an origin unconnected with crystallization. The laminæ of talc, found on the cleavage planes, he thinks were formed after cleavage was produced. Mr. Sharpe is a strong advocate for the close connection between cleavage and foliation. He observes, that, in the highlands of Scotland, a slate, in which both cleavage and bedding are equally well defined, rests upon mica schist; while the structure, which in the slate is cleavage, passes into the schist as foliation, — the two structures combining at the junction: and that the same divisional planes continue through clay-slate and mica schist, from Loch Katrine to Glen Shea, being alternately planes of cleavage in the slate, and of foliation in the schist; the boundary lines between the formations being irregular, while the structural planes keep a direct course, passing several times from one rock to the other.[1]

Murchison and Geike contradict altogether the statements of Mr. Sharpe, and observe that, where cleavage does exist in the region above referred to in Scotland, it is transverse to those laminæ which Mr. Sharpe calls foliation, but which they call crystallized sedimentation; that, in the argillaceous schists on the flanks of the Grampians, the cleavage planes cut across the undulations of the black carbonaceous schist, — a result which, they hold, shows the crumpling up of the beds of shale by the same pressure which produced the cleavage: but they think the action which produced the cleavage was quite different from that which crystallized the original layers of the sediments, since the two structures cross each other; and they also consider the fact that cleavage planes are straight, while planes of foliation are so extremely convoluted, as evidence against similarity of origin.

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[1] Mr. Sharpe remarks, that the corrugations in gneiss and schist are most complicated where the inclination of the folia is the least; that where the foliation is perpendicular, it is arranged in straight planes; that the granitic varieties of gneiss, in which foliation is but slightly marked, are more common where the inclination is slight; while the well-marked separation of the minerals occurs where the gneiss is perpendicular, or highly inclined. — *Phil. Trans.*, 1852.

Mr. David Forbes, in a paper upon the foliated rocks of Norway and Scotland,[1] observes that foliation cannot be crystallized sedimentation, because the folia sometimes occur at an angle with the bedding. He believes cleavage to be a mechanical, but foliation a chemical operation, and therefore not produced by the same cause;[2] but he remarks, that foliation may occur after cleavage, in which case it would follow the planes of least resistance. He also thinks that the superincumbent pressure must be taken into account in considering the arrangement of the folia, as this would act against any vertical disposition of the crystals; and the elongated and flattened appearance of the crystalline minerals, inducing foliation, he considers the result of this action. Mr. Forbes also agrees with Murchison and Geike, that cleavage, being in straight planes, while foliation is often contorted, argues a difference of origin. In concluding, he says that foliation and cleavage are distinct, and that when parallel it results from a previously induced structure; that foliation is the result of a chemical action, combined with a simultaneous molecular arranging force, developed at a heat below semifusion; that, when we find a similar structure in rocks once fused, such structure has been developed since solidification; and that foliation tends to follow lines of least resistance, whether produced by cleavage or by stratification.[3]

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[1] Quart. Jour., 1855, vol. xi. p. 166–85.

[2] Mr. Jukes' Manual, p. 274, also holds, that, while a mechanical force may produce cleavage, it seems impossible that it could produce foliation, as the latter requires an alteration in the mineral arrangement; but we shall see hereafter, that *the pressure may cause chemical action.*

[3] Keilhau thinks that generally foliation agrees with bedding, but mentions cases where it stands at right angles to it. Mr. Ramsay (Quart. Jour. 1853, p. 172) agrees with Mr. Forbes, that, if the rocks be uncleaved when metamorphism takes place, the foliation will be apt to coincide with the bedding; but that, if cleavage has occurred, foliation may be expected to follow the direction of the cleavage planes. Mr. Lyell also concludes, "that, in rocks where no cleavage has intervened, foliation and the planes of stratification will usually coincide;" but he

Messrs. Harkness and Blyth, in a paper upon the cleavage of the Devonians of the south-west of Ireland, give chemical analyses of various rocks subjected to cleavage, from which it is shown, that beds of the greatest specific gravity, and which contain the greatest amount of alumina, have the best-developed cleavage; while, as the rock has more silica and approaches to the nature of sandstone, cleavage is less plainly developed. A remarkable fact mentioned in this paper is, that, in Valentia island, the cleavage occurs in very complex flexures; as much so, indeed, as foliation does. Another fact, confirming the idea that pressure has caused the cleavage is, that, where the cleavage is at a high angle with the bedding, the adjacent beds are squeezed into each other so as to form one rock. The paper also confirms the statement that the axis of disturbance and the strike of the cleavage planes are parallel. The authors observe that the cleavage seems to be intimately connected with the "rolls" of the strata, being more perfect where these "rolls" occur. Finally, they state that the various phenomena support the conclusion, that pressure has produced the cleavage; while the strike of the cleavage planes and of the strata being alike, shows that the same force which produced the waves or folds in the strata originated the cleavage. Not only are the strata in this region thrown into waves, but the waves have a prevailing steeper dip on the north sides; and, correspondingly, the cleavage planes have all a more or less south inclination: and it is remarked, that the cleavage does not result from a simple "rolling" of the strata, but from this cause combined with a considerable amount of pressure, which, acting from the south, has pushed the strata over in a series of oblique curves to the north, and given to the inclined cleavage its more

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admits, "that the alternate layers of quartz and mica are more distinct in certain metamorphic rocks than the ingredients composing alternate layers in most sedimentary deposits; so that similar particles must be supposed to have exerted a molecular attraction for each other, and to have congregated together in layers more distinct in mineral composition than before they were crystallized." — *Man.*, *Am.* ed., 1861, p. 607–9.

or less southern dip. As a final conclusion it is stated, that "every circumstance attendant upon the cleavage of the rocks, as this is seen in the south-west of Ireland, tends to support the deductions of Mr. Sharpe, that there has been a compression perpendicular to the planes of cleavage, and expansion along those planes."[1]

In the Report of the British Association for 1856, Professor John Phillips presents an elaborate paper on the subject of cleavage, in which it is stated, that this structure is symmetrically related to the axis of movement of the strata, but that it maintains a more uniform direction than the beds do, and is thus due to a more general cause than that which made the folds; that the planes are always parallel to the main direction of the great anticlinal axes, but are not affected by the small undulations and contortions of those lines; that, when beds of unequal hardness lie adjacent, the planes traverse the harder beds in a direction more nearly perpendicular to the stratification than in the softer ones; and, even when slate beds contain nodules of quartz, the cleavage, which, in the slate is perfect and continuous, becomes irregular cracks, crossing the nodules in such more perpendicular direction.

Mr. Sorby has perhaps done more to elucidate, in detail, the structure of the cleaved rocks than any one else.[2] Many of the fine-grained slates, he says, contain minute rounded grains of mica, seldom over one one-hundredth of an inch in diameter, which, under the microscope, are seen to be composed of many laminæ, which, when they lie parallel to the cleavage planes, are undisturbed; but, when perpendicular to those planes, are distorted and bent. From the distortion of the included minerals and fossils, Mr. Sorby concludes that the average condensation of fine-grained slates has been to about one-half of the original volume. In a fragment of unaltered Devonian limestone, magnified two hundred times (linear), he shows the cells in an encrinite joint in the form of regular hexagons, while, in a highly cleaved limestone, the cells are all flattened out in the line of cleavage. In uncleaved limestones, calc-crystals, filling the cavities of organic bodies, have their crystalline cleavage planes almost invariably straight; while, in a highly cleaved specimen, the calc-spar is bent and crushed, — showing that the compressing force acted so intensely and so gradually as to change the molecular arrangement even of calc-spar, and bend it. Mr. Sorby observes, that the effect of pressure is to arrange the minute particles with their longer dimensions in the plane of cleavage. A plane of structural weakness is thus produced, which gives to the slate its fissile character. In the most perfect slates, he says, the cleavage planes are merely fractures passing amongst the particles. To this Mr. Tyndall objects, and states that he produced the most perfect cleavage possible, by compressing a mass of pure white wax. Mr. Sorby replies, that wax melted in a thin sheet, and examined with a microscope, is seen to be composed of prismatic crystals from one one-thousandth to one three-thousandth of an inch long, and from one five-thousandth to one ten-thousandth of an inch wide, arranged in radiating groups; and that it is just such a structure as, when compressed, would

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[1] Mr. Sharpe's first paper was read Dec. 2, 1846, and published in the Quart. Jour. for 1847. Bauer, in Karstens Archiv., 1846, vol. xx., suggested that pressure, resulting in an internal expansion, was the cause of cleavage. "Bei der Dislocation des Rheinischen Schiefergebirges wurde dasselbe einem gewaltigen Druck unterworfen, der von Süden nach Norden wirkte; dieser Druck erzeugte die Sattel und Mulden, die Ueberschiefungen und Verwerfungen und brachte in den Schichten eine innere Spannung hervor, welche die Ursache der Schieferung ist. Die Richtung der Schieferung musste sich durch die Richtung des Druckes bestimmen, auf welcher sie möglichst rechtwinkelig ist: da sich nun die Richtung des Druckes im Ganzen gleich blieb, so erklärt sich daraus der auffallende Parallelismus der Schieferung über grosse Räume." — *Naumann's Lehr.*, vol. i. p. 955.

[2] We can hardly do justice to the minute and patient labors of Mr. Sorby in the microscopic study of rocks. He has fairly proved, that the

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vast forces which have given rise to mountains may be traced in the minutest particles, invisible to the naked eye. He examines the included fragments one ten-thousandth of an inch in diameter, and shows us to what actions they have been exposed.

give rise to the most perfect cleavage. To this Mr. Tyndall answers, that, if the crystals of wax were *pulverized* and then compressed, the cleavage would be just as good. Mr. Sorby *has seen* the minute structure and the arrangement of unequiaxed particles, as far as microscopic vision can penetrate, while Mr. Tyndall is assuming a condition to suit his views. Mr. Sorby does not consider mica a *necessary* element in the production of cleavage, though he thinks that it is very generally instrumental. What he insists upon is, that cleavage is due to mechanical causes; that cleaved rocks are compressed rocks; and that the compression has not only changed the arrangement of the unequiaxed particles, but in some cases has altered their form. The Penrhyn and Llanberis slates contain about half their bulk in mica flakes, — about one one-thousandth of an inch long, and one ten-thousandth of an inch thick, — which Mr. Sorby does not regard as deposited in the form of mica, but as made by a peculiar transformation of a clay deposit. He also observes, that cleavage cannot depend upon any crystalline polarity, since the best-cleaved limestones, tested by the polariscope, exhibit no polarity whatever; that crystals are found with their axes in all directions; and that, when a rock is completely crystalline from chemical changes, there is no cleavage. Still he thinks that crystalline action might cause layers of different mineral composition to be formed in such a manner as to produce such cleavage as seen in some foliated rocks.

Mr. Rogers, at the conclusion of his paper at first referred to, calls attention to some important facts in regard to cleavage, and proposes to account for the phenomena in a manner different from any of the preceding. He remarks that the cleavage planes are generally parallel to the average dip of the anticlinal and synclinal axis planes, or those bisecting the flexures; but that when cleavage is fully developed, and the flexures are sharp and conspicuous, the cleavage planes immediately adjoining those bendings are not parallel to the axis planes, but partially radiate from them in a fan-like arrangement, upward in the anticlinals, and downward in the synclinals. He also notices the change of direction when cleavage planes pass from a harder to a softer bed, and the influence of mineral texture; but objects to the pressure-theory advocated by Sharpe, Phillips, Sorby, Tyndall, Daubrée, and others, and prefers one of his own, which may be considered as combining the ideas of Sedgwick and Herschel;[1] inasmuch as he attributes cleavage to "waves of heat" transmitted through the strata, which soften and polarize the matter, arranging the particles in parallel planes, perpendicular to such waves. He also considers cleavage and foliation to have originated at the same time, from the same cause, and to be different stages of the same crystalline condition. When large tracts of metamorphic rocks are nearly level, the foliation is stated to be almost invariably coincident with the bedding; in which case the waves of heat flowed upward through the crust, invading bed after bed in parallel planes. But, when injections of granite occur in uplifted strata, the crystalline lamination will be highly inclined, because the waves of heat will flow off in a horizontal direction from the injected mass.[2] This idea, Mr. Rogers thinks, is confirmed by the occurrence of cleavage parallel to the walls of (supposed) highly heated fissures. He remarks that the hard argillaceous shales show cleavage invariably parallel to the general plane of the fracture, and observes that such fissures would be the channels through which the heated volcanic steam would ascend from the interior, and that the action of this would be the same as that of a molten dyke; *i.e.*, transmitting a wave of heat perpendicularly to its surface, softening and polarizing the matter as it passed. Mr. Phillips refers to these same vertical divisional planes parallel to the surfaces of dykes, but observes, that, notwithstanding the resemblance to cleavage, the structure is distinct; being simply joints, but not farther cleavable as slates are.[1] Mr. Sedgwick also remarks a wide difference between joints and true cleavage planes.[2] It is hard to understand how the structure adjoining a dyke should be, in some cases, in planes parallel to the dyke; in others, in prisms perpendicular to the plane of the fissure; and, in still other cases, how both should occur at once: yet it seems to be so.[3] Mr. Rogers strenuously objects to the idea that cleavage has been produced by compression, which, considering the evidence, is to us an incomprehensible thing. The fact that cleavage is confined to the fine-grained rocks, and is not well developed in coarse sandstones, grits, &c., Mr. Rogers thinks sufficient to refute the purely mechanical origin of the structure; for, he says, we cannot conceive how a mechanical force, transmitted equally through parallel layers of coarse and fine materials, should cleave the fine argillaceous beds, and stop short at the coarse ones. To which objection it has been replied,[4] that, for cleavage to occur, we must have cohesive force to hold the particles together, which is lacking in the sandy beds, but exists in the clay. To Mr. Rogers's objection, that a uniform force, acting through the curved surfaces of the strata, would not produce a system of planes parallel among themselves, and regardless of the waves of the beds, it is answered, that while the strata admit of yielding, as they must do in the process of contortion, the molecular action will be slight: but when the general yielding has ceased, then the molecular operation will be greater; and, if the beds are rigid, the pressure will be propagated through them, regardless of their form; and it is suggested, that, while heat facilitated the molecular movement, pressure determined its direction. Mr. Rogers next objects, that pressure would produce a uniform condition of aggregation, and not the regular alternation of a greater and less condensation of particles, which give rise to the planes of less cohesion. This is an objection which at first sight would seem just; but we see no reason why it should not apply as well to Mr. Rogers's hypothesis. Why should uniform passage of heat produce an intermittent result, any more than uniform pressure? But that a gradual, uniform pressure does produce intermittent results, we have abundant evidence. Mr. Tyndall produced a perfect cleavage in pure white wax, by simple compression. The "blue bands" in the ice of glaciers is evidence to the same point.[1]

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[1] Herschel suggested, that if rocks are heated enough to allow the parts to move among themselves, or at least upon their own axes, some general law will determine the position in which they will cool; and that probably that position will have relation to the direction in which the heat escapes. — *Lyell's Manual, p.* 602–3.

[2] In his paper on the origin of the Appalachian coal strata (Trans. Assoc. Am. Geol., 1840–42, p. 473), Mr. Rogers attributes the debituminization of coal to a "prodigious quantity of intensely heated steam and gaseous matter emitted through the crust of the earth."

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[1] Report on Cleavage in Proc. Brit. Assoc., 1856, pp. 394-5.

[2] "Large Mineral Masses." Trans. Geol. Soc., vol. iii. part iii. p. 461.

[3] Phillips on Malvern Hills, Mem. Geol. Sur., before referred to, pp. 156-7.

Naumann, Lehr., vol. i. p. 957, says, "Diese Zerklüftung darf wohl nicht mit der plattenförmigen Absonderung identificirt werden, von welcher sie sich dadurch unterscheidet, dass die Klüfte eine weit grössere Ausdehnung besitzen, auch gewöhnlich in grösseren Intervallen auftreten, und bei geschichteten Gesteinen die Schichten mehr oder weniger rechtwinkelig durchschneiden."

Mr. Sorby (Proc. Brit. Assoc., 1857, p. 92, Trans. of Secs.) calls this "close joint cleavage," and says that the particles are not arranged parallel to such joints, but are often related to entirely different planes.

[4] Portlock's Address to the Geol. Soc. for 1857, in Quart. Jour., vol. xiii.

Daubrée experimented upon compression as a cause of cleavage, and showed plainly that compressed clay acquired a very pronounced structure; but that, for this, two conditions were necessary: First, the substance pressed must be free to expand at right angles to the pressure; clay confined, and then compressed, became solid, but not laminated. Second, the mass must be just so plastic: if too dry, it breaks; if too wet, it laminates; but the laminæ cannot be separated. Specimens of the same clay of different degrees of dryness, simultaneously compressed, furnished separate layers, — some being made "schistose," and others not. Iron slags, treated in the same way while in a pasty state, did not become schistose, but a section showed differently colored veins alligned normally to the pressure. M. Daubrée concludes that the cleavage structure is due to mechanical action, chiefly energetic pressure; and that it is to a gliding movement (*glissements*), resulting from this pressure, that the cleavage is due.[1]

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[1] Mr. Rogers has adduced this very phenomenon to confirm his hypothesis. He conceives that "waves of heat," radiated from the rocky walls which bound the glacier, have induced the "ribbon structure;" which, near the sides of the glacier, he states to be "almost absolutely parallel with its mountain walls." He even goes so far as to say that the looped or festooned form of the "blue bands" is caused "in part by the downward tendency of the movement or flow of the semiplastic ice, and in part by the influence of the terminal moraine to induce that parallelism to itself, which the rocky sides of the glacier produce in the ice near them." The general fact regarding the ribbon structure, he says, is its parallelism "to the surfaces of higher temperature which enclose the glaciers" (Edin. Trans. xxi., p. 451, note). Since Mr. Tyndall's investigation, the above explanation will hardly be accepted.

CONCLUSION REGARDING CLEAVAGE AND FOLIATION.

There seems to be no doubt about the facts that cleavage is best developed in fine-grained clay rocks (and we should judge also from Daubrée's experiments, that a certain amount of moisture was necessary); that the planes sometimes coincide with, and sometimes cross the bedding, but generally stand at a high angle with it; that it is connected with disturbed regions, and strikes parallel to the main axis of elevation; and thus, as Mr. Dana observes, is intimately connected with the origin of mountains.[2] The included fossils and the included fragments demonstrate, beyond a shadow of doubt, what the experiments of Tyndall and Daubrée confirm, that cleaved rocks are rocks which have been strongly compressed in a direction perpendicular to the cleavage planes. The researches of Mr. Sorby show a general parallelism of unequiaxed particles in the cleaved rocks, which may be particles that were in the rock before, and were brought into that position by the pressure, and the movement at right angles to such pressure; or may have been developed in that position from the homogeneous matter of the rock, by the force producing the cleavage. With regard to foliation, it is sometimes parallel to, and sometimes at a high angle with bedding, but generally parallel to cleavage when near to it. Messrs. Darwin and Rogers believe both to be the result of the same cause; which in cleavage is partially, but in foliation fully, developed. Mr. Sharpe thinks that the action which produced cleavage in the slate produced foliation in the schist. Some geologists have supposed that foliation was no more than sedimentary layers crystallized. David Forbes and others object to this, that it is found at a high angle to bedding. The alternate folia of different mineral character, Mr. Darwin shows, can come from a homogeneous rock, without any fusion; and Daubrée produced, by pressure, bands of different color in semi-fluid iron slag. Mr. Hunt has shown, that the unaltered beds of the lower part of the Appalachians contain the same mineral substances, and in the same proportions, as the altered crystalline and subcrystalline beds of the adjacent region, where the segregated crystalline minerals become palpable and visible.[1] Mr. Hitchcock states that cleavage passes insensibly into foliation, and he considers the folia of certain schists to be developed by pressure *and* chemical action.[2] David Forbes suggests that cleavage is due to mechanical, but foliation to chemical action; and that the latter follows the planes of weak cohesion which it finds prepared. Thus, if it finds a rock which has been compressed enough to produce a cleavage structure, it follows such structure; but that otherwise it follows bedding. To this it may be said that foliation occurs in a crumpled form, while cleavage planes are straight, and that the planes of sedimentary deposition are not corrugated like foliation. There are in foliation two things to be accounted for, — the alternating folia of different mineral composition, and the crumpled form which so often accompanies it. The fact that these two sometimes occur together, but that often the latter is absent, would seem to argue a double origin; of which the members work sometimes together, and sometimes singly. Mr. Darwin saw at the Cape of Good Hope a junction between granite and clay-slate, where the latter was converted into a fine-grained, perfectly characterized gneiss, composed of felspar, quartz, and mica. The laminæ of the clay-slate were straight; but, as they assumed the character of gneiss, they become undulatory, with some of the smaller flexures angular, like the folia of many true metamorphic schists.[1] Mr. David Forbes observes, that many minerals apparently possess in themselves a certain arranging power; that minerals not only show themselves in the same crystalline form, but likewise have a tendency to arrange themselves in certain figures.[2] Professor Hausmann states, that sandstone, from a blast furnace in the Hartz, not only received a speckled appearance from the penetration of graphite, but that the originally solid mass was even converted into thin, undulatory, easily separable beds.[3] We may suppose, says Portlock, that while heat caused the molecular motion, pressure determined the direction which the new structure should take.[4] When gneiss occurs in large horizontal tracts, says Mr. Rogers, the foliation is parallel to the bedding. The superincumbent vertical pressure upon any rock, Mr. Forbes observes, would be likely to act against any vertical position of the crystals.[1] Almost every intermediate step from cleavage to foliation may be traced in the metamorphic rocks. In cleavage and foliation we see the effects of pressure, heat, and chemical action: we know that these three are convertible; and, strange enough, Sedgwick, Rogers, and Sharpe have severally called upon crystalline polarity, waves of heat, and pressure, separately, to account for the phenomena of cleavage and foliation.[2]

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[1] Études sur le Métamorphisme, p. 66.

[2] Am. Jour., vol. iii. p. 389.

Mr. Robert Hunt (Mem. Geol. Sur. Great Brit., vol. i. pp. 451–2) describes an experiment which he tried upon a cube of clay about a foot square. It was placed between the zinc and copper plates of a battery, and a current of voltaic electricity was passed through it for six months, when the side towards the zinc was laminated to the depth of an inch and a half, the remainder of the mass being irregularly distorted; but we are not informed as to the real nature of the lamination, or how the clay was prepared for the experiment. Mr. R. W. Fox also produced an imperfect lamination in wet clay, by a long-continued galvanic stream.

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[1] Quoted by Mr. Hall, Pal., p. 77.

[2] Report on Vermont, vol. ii. p. 593.

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[1] Darwin, Volc. Isles, p. 149–150.

[2] Quart. Jour., vol. xi., before referred to.

[3] Edin. Phil. Jour., vol. xxiv. p. 83.

[4] Address to Geol. Soc., before cited.

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[1] Foliation in Norway, &c., Quart. Jour., vol. xi.

[2] The reader is supposed to be acquainted with the recent developments of physical science, so far as to know that heat, motion, dynamical energy, chemical action, &c., are convertible one to another; that no one of these ever disappears, except to appear as some other one; that no one is ever annihilated, though it may elude our pursuit for a while. This mutual interaction of heat and pressure is an important thing to be observed; for there are many cases where an effect seems to have been produced by heat, when compression is the only cause that we can discover to have acted on the materials.

In 1843, Mr. Colding, chief engineer to the city of Copenhagen, presented a paper to the Royal Society of that place, in which he remarks, "As the forces of nature are something spiritual and immaterial, entities whereof we are cognizant only by their mastery over nature, these entities must of course be very superior to every thing material in the world; and, as it is obvious that it is through them only that the wisdom we perceive and admire in nature expresses itself, these powers must evidently be in relationship to the spiritual, immaterial, and intellectual Power itself that guides nature in its progress. But, if such is the case, it is consequently impossible to conceive of these forces as any thing naturally mortal or perishable: therefore, when and wherever force seems to vanish in performing certain mechanical, chemical, or other work, the force merely then undergoes a transformation, and re-appears in a new form, but of the original amount, as an active force." Descartes had long before (Prin. Philos.) used the following remarkable language: "Deum esse primariam motus causam, et eandem semper motus quantitatem in universo conservare." Professor Bohn objects to thus basing the proof of a theorem upon a divine attribute, and observes that "the assumption of a constant quantity of motion involves a limitation of divine power. The almightiness of God must manifest itself by actual achievement; therefore, assuming, with Descartes, the indestructibility of that which exists, the quantity of motion must increase." If we suppose the force, motion, heat, or whatever we choose to call it, which we

SORBY, ON THE CORRELATION OF FORCES IN GEOLOGY.

Mr. Sorby found, in his microscopic studies, facts which pointed both to a mechanical and a chemical origin; and, at first, he thought the two might be combined: but other facts, he says, turned up which precluded any supposition not involving direct correlation; for they most clearly indicated that mechanical force had been resolved into chemical action, in the same way as under other circumstances it may be resolved into heat. He found also, that pressure causes in some cases a slower, and in some cases a quicker, chemical action, which we might suppose would be the case, as the amount of water varies in different rocks; though we cannot say just what effect upon the action of pressure the included water would have. Mr. Sorby remarks, that a number of facts connected with the metamorphic rocks, and with the phenomena of cleavage, otherwise inexplicable, are readily explained, if mechanical force be directly correlated to chemical action, and if, in some cases, the direction in which crystals are formed is more or less related to pressure, in some such way as there is a connection between their structure and magnetism, as shown by the experiments of Plücker, Faraday, and Tyndall;[1] — a supposition which would seem to receive support from Mr. Sedgwick's observation, that semi-crystalline flakes strike in planes parallel to cleavage through

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see exerted about us, to be something created by God, and left to take care of itself, the above objection perhaps might hold: but if we believe that the active energy of nature depends, for its existence, upon a constant and inseparable connection with the divine intelligence; in fine, that *it is that divine intelligence* in its lowest form, — we shall see, as Professor Parsons has stated it (Essays, 1st ser.), that "*existence is perpetual creation.*" Dr. Mohr, of Coblentz, concludes, "Was . . . eine Kraft hervorbringt, muss selbst eine Kraft sein." — "In all phenomena," says Mr. Grove, "the more closely they are investigated, the more are we convinced, that, humanly speaking, neither matter nor force can be created or annihilated; and that an essential cause is unattainable. Causation is the will, creation the act, of God."

[1] Phil. Mag., 1864, vol. xxvii. pp. 145-53.

the whole mass of the cleaved rock. Mr. Phillips observes, that the laminæ and banded structure may be regarded as indications of crystallization under restraint; such restraint having reference to particular planes, in consequence of the pressure of preconsolidated parts adjacent, or of the flow of heat in particular directions.[1] Professor James Thomson remarks, that stresses tending to change the form of any crystals, in the saturated solutions from which they have been crystallized, must give them a tendency to dissolve away, and to generate, in substitution for themselves, other crystals, free from the applied stresses, or any equivalent ones.[2] Pressure would act according to the nature of the rock subjected to it. A rigid rock would transfer the force farther than a soft one. If the material acted upon is able to yield easily to pressure, heat will not be developed; but if it is rigid enough to resist a change of external form, the pressure will exert itself in altering the internal molecular arrangement of the mass. Even the same sediment, under different stages of consolidation, will be affected differently by the same pressure. We might suppose that moderate pressure would, in a given material, first develop cleavage (a pure mechanical structure); next, as the material became more compressed and less able to yield, a structure partaking both of a mechanical and chemical nature (transition) would begin to appear: and finally, when the mass had become so firm as to allow heat to accumulate, without any change of external form, it would show no direct evidence of pressure, but only of heat; yet the cause would have been, through the whole, the same and continuous; while the molecules of the solid rock would assume such new chemical and mineral forms, as, under the applied strain, would produce the most perfect equilibrium within the mass. The changes which take place in the internal structure of iron are induced by quick blows often repeated, the extreme form of which is compression. The oft-repeated blows develop heat, which results in producing a crystalline texture. Pressure also results in heat, and we may suppose with the same effect, though we have no direct evidence to the point. If we bear in mind the enormous pressure generated in the folding up of masses of rock, the depth of which is measured by miles, and the immense duration of time consumed, we shall have no difficulty in accounting for any forces, mechanical or chemical, which may be needed to produce the effects which we see in nature; and the wide limits to the amount and direction of these forces, at different times and places, acting upon sediments of every variety of mineral composition, could not result otherwise, than in the production of an infinite variety of results. As several geologists have suggested, the bending of the strata may give rise to fractures and fissures, which may be filled by sediments from above, or by rocks adjacent to such fissures, reduced to a plastic state, and forced into them; or even by the injection of mineral matter, as before suggested (note on page 98), which has been softened or melted by heat suddenly allowed to act, at a fracture, by the relief from pressure. Mr. Hall remarks, that the prime cause of metamorphism exists within the material itself, and that it is due to motion, or fermentation and pressure, aided by a moderate increase of temperature, producing chemical change.[1] Prof. Hitchcock regards the process, which changed conglomerates into schists and gneiss, as both mechanical and chemical; the pebbles being flattened into the siliceous laminæ of the schist, and the cement changed into the mica and talc, whose parallel structure probably resulted from pressure. He also says, that where we find rocks made schistose and foliated by mechanical action, there we may look for granite; and he considers the production of granite to be most intimately connected with the alteration of the sedimentary rocks; not as cause and effect, but as joint effects of a common cause.[1] This common cause would be mechanical compression, with the heat and chemical actions which proceed from it; and the same source would furnish to Keilhau his "tranquil processes," to Mr. Hunt his "molecular changes and chemical re-actions," to Mr. Sorby his correlated, mechanical, and chemical forces, and the internal "fermentation" of Mr. Hall.

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[1] Mem. Geol. Sur. Great Brit., vol. ii., part i. p. 46.

[2] Phil. Mag., 1862, vol. xxiv. p. 396. Professor Naumann (Lehr., vol. ii. p. 248) says, "Dass alle diese Metamorphosen bei ziemlich hoher Temperatur und starkem Drucke, unter Mitwirkung des Wassers hervorgebracht wurden, dafür sprechen die Experimente von Daubrée und die mikroskopischen Untersuchungen von Sorby. *Der Druck bildete jedenfalls einem sehr wichtigen Coefficienten bei dergleichen Operationen, weil ohne ihn die starke Ueberhitzung des Wassers nicht Statt finden konnte.*"

Professor Naumann classes ice, which he calls metamorphosed snow, among the rocks; and, from this view, we may pass to Mr. Tyndall's researches upon glaciers, and we shall have the ribboned or veined structure, as gneissoid or schistose ice: in which pressure, transformed into heat, has produced alternate laminæ of different mineral composition.

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[1] Pal., p. 77.

EVIDENCE OF PRESSURE IN METAMORPHIC REGIONS.

Abundant evidence can be collected to show, that cleavage, foliation, and the more extreme changes which produce the thoroughly crystalline massive rocks, are all intimately connected with regions of disturbance; and that the rocks in such tracts bear the most undoubted signs of severe compression. Mr. Hall observes, that, in approaching the Appalachians from the west, as the undulations of the strata become more extreme, "a gradual and almost imperceptible change" comes over the rocks. "The shale appears as if partially crushed or pressed; and shows striæ, and smooth, shining surfaces, which are not parallel to the line of bedding." It breaks into small irregular fragments, with blackened, striated, and shining surfaces. "The mass presents an appearance as if there had been a movement within itself, or as if it had been partially crushed by the folding, producing a sliding of the fragments over each other. This condition is not universal. Some portions still retain their lamination, breaking into large pieces, but with numerous faults; the faces of such faults showing smooth, glazed surfaces, while the continuation of the lamination, if traceable, is not found in the same line: and an intermediate space is often found filled with soft, crushed, shaly matter. Calcareous seams often accompany this condition of the shales. At the same time, the heavier arenaceous and argillo-arenaceous layers have become harder and more compact, changing somewhat in color, and developing crystalline matter in the joints." Mr. Hall observes that the gradual development of the segregated and crystallized minerals in the sedimentary rocks "is due to chemical action;" and that "the original character of the beds has determined the kind of mineral which shall be developed, when the mass is placed in condition to allow of free molecular motion."[1]

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[1] Why, after showing that a schist can result from the compression of a conglomerate, Mr. Hitchcock should suppose granite to be a sediment entirely fused by heat from below, and schist only partially fused, we cannot see; nor can such be the case, since the azoic schists underlie the granites. Daubrée and Sterry Hunt suppose that the various mineral beds are more or less saturated with alkaline waters; and will thus be changed by heat ascending from below, or will remain unaltered, as such saturation is or is not sufficient to furnish the re-agents, which the heat needs to produce the change. Mineral waters do certainly act in this way, as seen at Plombières; but we should prefer to get the heat needed, by the compression, which accompanies the disturbance of the strata where metamorphism occurs.

Professor Phillips, in his paper on the Malvern Hills, observes: "In arched stratification, when the beds are of very unequal extensibility, as the limestone and soft shale, the limestone is often found broken in detached spaces, which are filled with the crushed shales. On examining the surfaces of bent strata, they are often found striated, as if the parts had slid over each other. If the beds are much broken, these striæ run in several directions, corresponding to the several pressures; but, if they appear continuous, the striæ are usually parallel over large surfaces. Very extensible beds (as anthracite coal) are squeezed and altered in the thickness of the flexures."[2]

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[1] Pal., pp. 73, 74, and 94. [2] Mem. Geol. Sur., note to pp. 136-38.

Mr. Murchison says, "With the expansion and retrocession from the chief axis (of disturbance), and with the diminution in altitude, the crystalline character of the chain gradually disappears. The limestones part with their saccharoid and slaty aspect, the mica slates pass into micaceous flagstones, the quartz rocks into conglomerates and psammites; and thus the observer is regularly conducted, with few if any discordant or unconformable junctions, from a crystalline nucleus to ordinary sedimentary masses."[1]

Mr. Jukes, speaking of the Leinster district in Ireland, says that, in going towards the main granite ridge, the slates are found, sometimes at a great distance, to have acquired a "glaze," or a micaceous lustre, which is apparent throughout the mass when the slates are broken, and even when they are ground to sand or powder. This micaceous appearance increases as the granite is approached, until at last distinct plates and folia of mica are seen, and the whole assumes the ordinary character of mica schist, occasionally passing into a kind of gneiss, and often assuming the puckered and corrugated structure, which, he thinks, must be ascribed to a simple mechanical force compressing the rock laterally.[2]

Mr. Sorby says, that certain irregularities of bulging in coal-beds can be due to no other cause but that of a sliding crush, propagated through the rocks, coal and all, in a direction diagonal to both the original bedding and to the subsequent upheaval. Gneiss, says Scrope, is known often to show polished and striated surfaces, proving a sliding and slipping of its parts. In mica schists these marks of friction are yet more general and unmistakable.[3] The accumulation of a great thickness of strata, says Mr. Hunt, seems to have been a condition both of the corrugation and alteration of sedimentary rocks. Regions of altered strata appear to be, in all cases, folded or corrugated.[1]

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[1] Russia, &c., p. 462.

[2] Manual, pp. 275-76; and Brit. Proc., 1856, p. 68.

"Les Terrains métamorphiques," says Daubrée (Études, p. 117), "sont confinés exclusivement dans les régions disloquées. Les phyllades (*clay-slates*), qui ne sont que le premier terme de transformations plus profondes, ne se trouvent jamais en dehors de zones autrefois plus ou moins disloquées."

[3] "Volcanoes," note to p. 300, 2d ed., 1862.

CONCLUSIONS.

We have devoted a large part of the preceding pages to metamorphism, — to the examination of rocks in detail, which may seem like wandering from our main subject: but it is the details which make up the whole; and we conceive that a neglect of looking into the intimate structure of the materials with which we deal, has led to putting forth many hypotheses in geology, which, in reality, have no foundation in nature, and which violate known laws, and are contradicted by numerous facts. Between the first part of this paper, which notices the general movements and arrangements of the crust of the earth, and the second part, which examines the intimate structure of that crust, there is, we think, the relation of cause and effect. The deposition and subsidence of large accumulations of sediment produces folding and compression: the alteration of these sediments has arisen primarily from compression, but such force may be resolved into other actions, and thus give rise to phenomena which seem to be due to heat and to chemical action; and we hold it better philosophy to attribute such heat and chemical action to a force which we know has acted in just these regions, and nowhere else, and which will produce such results, than to a supposed central mass of fluid, gaseous emanations, and the like, that we know nothing about, which seem opposed by important facts, and which, from all we know, should act generally and not locally; and especially not in these regions of great accumulation, which from their very thickness would seem to be most removed from any source of heat beneath the earth's crust. We do not propose any hypothesis which shall explain every thing. We have simply reviewed a number of facts, and have made some suggestions, which, we think, may be followed up to advantage in studying the great orographic features of the globe. We regard the questions relating to the connection of the crust movements, and any deep-seated terrestrial operations, as entirely open; and, while the evidence that plication and metamorphism have resulted from the subsiding of large masses of sediment seems satisfactory, the actual *elevation* of mountains remains unaccounted for. From the latest discoveries, we seem obliged to conclude, that the entire solid crust of the globe, so far as we have been able to get access to it, consists of sedimentary rocks either altered or unaltered; but no satisfactory explanation of the origin of these sediments has been given. We regard the tendency to apply the recent developments of physical science to the investigation of geological phenomena, as in the highest degree favorable to progress. If Mr. Hall's theory makes our ideas less definite than some others which have preceded, it at least opens a fruitful field for research, in cultivating which we do not so violate the known laws of physics as we do by many of the older hypotheses.

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[1] Canada Geol. Survey, ch. xix. p. 584.

"It can only be amid a thousand errors," says La Bêche, "and by a determination to abandon our preconceived opinions when shown to be untenable, not by pertinaciously adhering to them because we have once adopted them, that we can approximate towards the truth. By strictly advocating a particular theory, prominently displaying the facts only which appear to afford it support, we are in perpetual danger of deceiving ourselves and others. Facts of all kinds, whether in favor of or against our views, should be honestly brought forward, in order that those whose opinions are unprejudiced may fairly weigh the evidence adduced."

NOTES.

I.

In the preceding pages, we have spoken of the stratified rocks in the continental basin, lying between the Appalachians and the Rocky Mountains, as being undisturbed and in a horizontal position. This, however, is not strictly correct. The Silurian, Devonian, and carboniferous formations in Ohio, Indiana, Illinois, and Missouri, lie in very broad and gentle undulations, — a good idea of which may be had by reference to Johnston's "Physical Atlas," plates vii. and viii., where generalized sections across the United States from east to west, prepared by Professor H. D. Rogers, are given. The undulations of these strata, however, are so gentle, as compared with the excessive folding of the same formations in Pennsylvania and Virginia, that the reader will hardly be misled by the statement that they are undisturbed. The idea which we intended to convey, viz., that, where the strata had a very great depth, they have been subjected to immense forces which have folded them closely; but that, where the depth was small, the forces have been comparatively gentle, — is entirely correct.

II.

Upon pages 24 and 25 we conclude, that, in past geological times, the crust of the earth must have been very flexible to have allowed the subsidence to take place which seems to have accompanied the deposition of sediments. In the note to page 48, we find it difficult to understand how the subsidence could commence. And upon pages 52 and 53 we conclude, that no beds of sedimentary matter with which we are acquainted could have forced down a crust of even twenty-five miles in thickness. The inference seems to be, that while the subsidence has in some way depended upon the amount of accumulated sediments, since the thickness of the accumulation is the measure of the settling, the weight of the deposits was not the force that caused the subsiding, or that flexed the crust. This is one of the many dilemmas in which the student will find himself. It should also be borne in mind, that the very region where the great Appalachian accumulations have occurred is the same as that where Mr. Dana's "submarine ridge" was formed before these accumulations commenced.

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